

## **EXHIBIT B**

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10 *State of California and Defendant Allison*  
*Mendoza in her official capacity as Director*  
11 *of the Bureau of Firearms*

12 IN THE UNITED STATES DISTRICT COURT  
13 FOR THE SOUTHERN DISTRICT OF CALIFORNIA  
14  
15

16 **CLAIRE RICHARDS, ET AL.,**

17 Plaintiffs,

18 v.  
19

20 **ROB BONTA, IN HIS OFFICIAL CAPACITY**  
**AS ATTORNEY GENERAL OF**  
21 **CALIFORNIA, ET AL.,**

22 Defendants.  
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Case No. 3:23-CV-00793

**EXPERT REPORT AND  
DECLARATION OF PROFESSOR  
CHRISTOPHER POLIQUIN**



## EXPERT REPORT AND DECLARATION OF PROFESSOR CHRISTOPHER POLIQUIN

I, Professor Christopher Poliquin, declare under the penalty of perjury that the following is true and correct:

The California Department of Justice has asked me to provide an expert opinion pertaining to firearms waiting periods and related restrictions in the United States in the above-captioned matter. This expert report and declaration (“Declaration”) provides that opinion and is based on my own personal knowledge and experience; if I am called as a witness, I could and would testify competently to the truth of the matters discussed in this Declaration.

### BACKGROUND AND QUALIFICATIONS

1. I am a scholar whose work spans the fields of public policy, with a focus on gun policy and its impact on violence; organizational design and strategy; and technology.

2. Since 2018, I have served as an Assistant Professor at the UCLA Anderson School of Management. I joined UCLA’s faculty after obtaining my Doctor of Business Administration from Harvard Business School and my bachelor’s degree from the University of Pennsylvania. A true and correct copy of my curriculum vitae is attached as **Exhibit 1** to this declaration.

3. In addition to my other academic work, I have published multiple peer-reviewed papers on firearms policy and violence. These include *Handgun Waiting Periods Reduce Gun Deaths*, published in the Proceedings of the National Academy of Sciences in 2017 and attached as **Exhibit 2**, and *The Impact of Mass Shootings on Gun Policy*, published in the Journal of Public Economics in 2020.

4. I have previously provided expert testimony on similar issues at a preliminary injunction hearing in *Rocky Mountain Gun Owners v. Polis*, No. 23-cv-02563-JLK (D. Colo.).

### RETENTION AND COMPENSATION

5. I have been retained by the California Department of Justice to render expert opinions in this case. I am being compensated at a rate of \$250 an hour for my work on this matter. My compensation is not contingent on the results of my expert analysis or the substance of my opinions or testimony in this matter.

### **BASIS FOR OPINION AND MATERIALS CONSIDERED**

6. I have been retained by the California Department of Justice to provide my expert opinion on the impact of firearm waiting periods on firearm deaths and violence more broadly.

7. Counsel for Defendants provided me with the operative Complaint in this matter and copies of the relevant statutes being challenged. Apart from these documents, my report is based on my independent research.

### **OPINIONS**

8. Based on my extensive review and analysis of the relevant evidence, it is my professional opinion that:

- Waiting period laws that delay the purchase of firearms reduce firearm-related homicides.
- Waiting period laws reduce firearm-related suicides, especially among young people.
- Reductions in gun homicides from waiting period laws are not offset by a concomitant increase in non-gun violence.

#### **I. STUDY OF WAITING PERIOD LAWS**

9. In 2017, my co-authors at Harvard Business School and I published a peer-reviewed study—attached as **Exhibit 2**—titled *Handgun Waiting Periods Reduce Gun Deaths* in the Proceedings of the National Academy of Sciences. This study examines 45 years of data between 1970 and 2014 on injury-related death and waiting period laws across all 50 states and the District of Columbia (DC). During this time, 44 states (including DC) had an effective waiting period for purchasing a handgun for at least some years.

10. Variation in waiting period laws across states and over time allows my co-authors and I to use modern, quasi-experimental statistical methods to estimate how waiting period laws affect violence. These estimates—which compare how violence changes within states that adopted or were forced to adopt waiting period laws to changes in other states—have a causal interpretation. In other words, our study design allows us to determine if waiting periods cause changes in violence and estimate the magnitude of any effects.

## II. THE IMPACT OF FIREARM WAITING PERIOD LAWS ON HOMICIDES

11. According to my study, waiting period laws cause large and statistically significant reductions in firearm-related homicides. Implementation of a handgun waiting period causes an approximately 17 percent reduction in gun homicides (Table 1, **Exhibit 2**). For California, this estimate suggests that the state avoids about 260 gun homicides per year due to its waiting period policy.

12. This estimate for the effect of waiting periods on gun homicides holds across several statistical models and time periods. It is insensitive to whether the statistical model includes a state-specific time trend or controls for alcohol consumption, poverty, income, urbanization, black population, population in seven age groups, and several other gun policies that may have been enacted coincident with laws that delayed the purchase of a handgun.<sup>1</sup> The estimate is also similar for both the full study period—1970 through 2014—and for the “Brady interim period,” a period from November 1993 to November 1998 in which federal law created a new, 5-day waiting period in 19 states to allow for the completion of a background check before firearm sales by federally licensed firearm dealers (Table 2, **Exhibit 2**).<sup>2</sup>

<sup>1</sup> Several of these additional analyses appear in an appendix to the article *Handgun Waiting Periods Reduce Gun Deaths*. The appendix, titled “Supporting Information,” is attached as **Exhibit 3**.

<sup>2</sup> Brady Handgun Violence Prevention Act, Pub. L. No. 103-159 (1993).

13. Reductions in gun homicides from the implementation of handgun waiting periods are not offset by increases in non-gun homicide. My estimates for the effect of waiting periods on non-gun homicide are close to zero and not statistically significant, which suggests waiting periods have little, if any, effect on rates of non-gun homicide (Tables 1–2, **Exhibit 2**).

14. Recent research has supported these findings. A study of handgun purchases during a spike in gun sales and subsequent homicides found that purchasing delays of various forms decreased handgun homicides, and that this decrease was driven by a decrease in “domestic violence and other heat-of-the-moment murders.”<sup>3</sup>

15. All of these results are consistent with research indicating that certain forms of interpersonal violence—especially domestic violence—are impulsive. In 2011, for example, researchers found statistically significant increases in police reports of family violence in particular locations immediately after the home professional football team suffered an upset loss.<sup>4</sup>

### III. THE IMPACT OF FIREARM WAITING PERIOD LAWS ON SUICIDES

16. Waiting period laws likely reduce firearm-related suicides. According to some analyses, the implementation of a waiting period causes a statistically significant reduction in gun suicide of 7–11 percent (Table 1, **Exhibit 2**). For California, a 7 percent reduction is equivalent to about 110 fewer gun suicides per year. During the “Brady interim period,” my co-authors and I estimate that waiting periods caused a 5–6 percent reduction in gun suicide (Table 2, **Exhibit 2**).

17. My estimates of the effect of waiting periods on gun suicides and total suicides (gun and non-gun) are more sensitive to the inclusion of control variables

<sup>3</sup> Christoph Koenig & David Schindler, *Impulse Purchases, Gun Ownership, and Homicides: Evidence from a Firearm Demand Shock*, The Review of Economics and Statistics 2023; 105 (5): 1271–1286.

<sup>4</sup> David Card and Gordon B. Dahl, *Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior*, The Quarterly Journal of Economics 126, no. 1 (2011): 103–143.

1 and the years examined than the estimates for homicide. While analyses of total  
 2 suicides for the period 1970–2014 suggest that waiting periods reduce all suicides  
 3 by a statistically significant 2–7 percent, estimates for the “Brady interim period”  
 4 suggest reductions of 2–3.5 percent and are less precisely estimated (Tables 1–2,  
 5 **Exhibit 2**).

6 18. Recent research—attached as **Exhibit 4**—has expanded on my 2017  
 7 paper *Handgun Waiting Periods Reduce Gun Deaths* by examining the effects of  
 8 waiting period laws on suicide for different age groups and by using newer  
 9 statistical methods developed after my own study.<sup>5</sup> The authors note that  
 10 impulsivity differs by age and that waiting period laws, which arguably disrupt  
 11 impulsive suicide, may therefore have different effects on different age groups.  
 12 Their analyses show that handgun waiting periods reduce gun suicide in the total  
 13 adult population by 3.3 percent, and reduce gun suicide among young adults, which  
 14 they define as people between the ages of 21 and 34, by 6.1 percent. The authors’  
 15 estimates for older adults suggest no statistically significant effect of waiting  
 16 periods on gun suicide. They report no effect of waiting periods on non-gun  
 17 suicide.

18 19. These results are consistent with other studies indicating that suicide is  
 19 often an impulsive act. Research has found that there is often limited time between  
 20 the formation of the intent and the completion of the attempt.<sup>6</sup> One study based on  
 21 interviews with those who have survived a suicide attempt, for example, found that  
 22 less than 10% of interviewees waited for more than 7 days after deciding to commit  
 23 suicide before taking the suicidal act.<sup>7</sup> And other research has found that suicidal

24 <sup>5</sup> Donohue, John J., Samuel V. Cai, and Arjun Ravi. *Age and Suicide*  
 25 *Impulsivity: Evidence from Handgun Purchase Delay Laws*. National Bureau of  
 Economic Research (2023). (Attached as **Exhibit 4**.)

26 <sup>6</sup> See, e.g., Megan Spokas, *et al.*, *Characteristics of Individuals Who Make*  
 27 *Impulsive Suicide Attempts*, *J. Affective Disorders*, 2021 Feb; 136(3): 1121-1125.

28 <sup>7</sup> Elizabeth A. Deisenhammer, *et al.*, *The Duration of the Suicidal Process:*  
*How Much Time Is Left for Intervention Between Consideration and*


1 impulses are unlikely to recur: those who survive an initial suicide attempt, for  
2 example, usually do not die by suicide later in life.<sup>8</sup>

3 20. My professional opinion, based on my own and other scholars'  
4 research, is that waiting period laws significantly reduce firearm-related homicides  
5 and likely reduce gun suicides as well, especially among young people, but that  
6 there is mixed evidence regarding the extent to which these reductions are offset by  
7 increases in non-gun suicides.

8  
9 **DECLARATION UNDER PENALTY OF PERJURY PURSUANT TO 28**  
10 **U.S.C. § 1746**

11 I declare under penalty of perjury that the foregoing is true and correct, and if  
12 called as a witness would testify competently to the above.

13  
14 Executed in Los Angeles, California on March 11, 2024.

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17 \_\_\_\_\_  
18 Christopher Poliquin

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28 *Accomplishment of a Suicide Attempt?*, J. Clinical Psychiatry, 2009 Jan; 70(1):19-24.

<sup>8</sup> See, e.g., Owens, et al., *Fatal and Non-Fatal Repetition of Self-Harm: Systematic Review*. British Journal of Psychiatry. 2002; 181(3): 193-199;



## Christopher W. Poliquin

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### Academic Employment

July 2018 — UCLA Anderson School of Management  
Assistant Professor

### Education

May 2018 Harvard Business School  
Doctor of Business Administration

May 2009 University of Pennsylvania  
B.A. Philosophy, Politics, and Economics

### Research

#### *Peer Reviewed Journal Articles*

- Chauvin, Jasmina and Christopher Poliquin. 2023. Supply-side inducements and resource redeployment in multi-unit firms. *Accepted at Strategic Management Journal*.
- Lawrence, Megan and Christopher Poliquin. 2023. The growth of hierarchy in organizations: Managing knowledge scope. *Strategic Management Journal*. 44(13): 3155–3184.
- Hou, Young and Christopher Poliquin. 2023. The effects of CEO activism: Partisan consumer behavior and its duration. *Strategic Management Journal*. 44(3): 672–703.
- Luca, Michael, Deepak Malhotra, and Christopher Poliquin. 2020. The impact of mass shootings on gun policy. *Journal of Public Economics*. 181(January 2020).
- Licht, Amir, Christopher Poliquin, Jordan I. Siegel, and Xi Li. 2018. What makes the bonding stick? A natural experiment testing the legal bonding hypothesis. *Journal of Financial Economics*. 129(2) (August): 329–356.
- Luca, Michael, Deepak Malhotra, and Christopher Poliquin. 2017. Handgun waiting periods reduce gun deaths. *Proceedings of the National Academy of Sciences*. 114(46): 12162–12165.

#### *Journal Articles Submitted*

- Chauvin, Jasmina, Carlos Inoue, and Christopher Poliquin. 2023. Resource redeployment as an entry advantage in resource poor settings. *Minor Revision at Strategic Management Journal*.
- Poliquin, Christopher. 2020. The wage and inequality impacts of broadband internet. *Revise and Resubmit at Management Science*.
- Poliquin, Christopher and Young Hou. 2023. Policymaker responses to CEO activism. *Revise and Resubmit at Organization Science*.
- Poliquin, Christopher, Megan Lawrence, and Samina Karim. 2023. Hierarchy expansion in young firms: The impact of internal versus external hiring on performance. *Revise and Resubmit at Management Science*.
- Hou, Young and Christopher Poliquin. 2024. Political consumerism: Ideology or signaling?

#### *Working Papers*

- Hou, Young and Christopher Poliquin. 2023. CEO activism and public mobilization.
- Poliquin, Christopher and Young Hou. 2022. The value of corporate political donations: Evidence from the capitol riot.

#### *Chapters in Books*

- Baron, Jonathan, William T. McEnroe, and Christopher Poliquin. 2012. Citizens' perceptions and the disconnect between economics and regulatory policy. In *Regulatory Breakdown: The Crisis of Confidence in U.S. Regulation*. Ed. Cary Coglianese. Philadelphia, PA: University of Pennsylvania Press. 143–162.

*Published Conference Proceedings*

- Poliquin, Christopher and Young Hou. 2022. The value of corporate political donations: Evidence from the capitol riot. *Academy of Management Proceedings*. Vol. 1.
- Chauvin et al. 2021. Unpacking internal mobility: Drivers and consequences of employee redeployment inside organizations. *Academy of Management Proceedings*. Vol. 1.
- Chauvin, Jasmina and Christopher Poliquin. 2019. Knowledge sharing and intra-organizational worker mobility. *Academy of Management Proceedings*. Vol. 1.
- Lawrence, Megan Lynn and Christopher Poliquin. 2019. Prior experience and the emergence of hierarchy in young firms. *Academy of Management Proceedings*. Vol. 1.

*Non-Peer Reviewed Publications*

- Poliquin, Christopher and Young Hou. 2023. How policymakers respond to CEO activism. *Blue Sky Blog*. Columbia Law School. 11 December.
- Poliquin, Christopher. 2022. After mass shootings like Uvalde, national gun control fails — but states often loosen gun laws. *The Conversation*. 25 May.
- Poliquin, Christopher and Young Hou. 2022. Corporate political donations and firm value following the Capitol riot. *The FinReg Blog*. Duke University School of Law. 10 February.
- Poliquin, Christopher. 2021. Gun control fails quickly in Congress after each mass shooting, but states often act — including to loosen gun laws. *The Conversation*. 22 March.
- Luca, Michael, Deepak Malhotra, and Christopher Poliquin. 2018. The case for handgun waiting periods. *Behavioral Scientist*. 12 December.

**Grants**

2019	Morrison Family Center for Marketing Studies and Data Analytics (with Young Hou) UCLA Anderson School of Management
2015	Pellegrini Summer International Economics Research Grant (with Jasmina Chauvin) Harvard Economics Department

**Teaching**

Winter 2024	Full-time MBA Program, UCLA Anderson Business Strategy (MGMT 420-1/2/3/4)
Winter 2023	Full-time MBA Program, UCLA Anderson Business Strategy (MGMT 420-1/2/3/4/5)
Winter 2022	Full-time MBA Program, UCLA Anderson Business Strategy (MGMT 420-1/2/3/4/5)
Winter 2021	Full-time MBA Program, UCLA Anderson Business Strategy (MGMT 420-1/2/3/4/5)
Winter 2020	Full-time MBA Program, UCLA Anderson Business Strategy (MGMT 420-1/2/5)
Fall 2018	Fully Employed MBA Program, UCLA Anderson Business Strategy (MGMT 420-3/4)
Spring 2015	Harvard John F. Kennedy School of Government Teaching Fellow, Economic Analysis of Public Policy

**Case Studies and Course Materials**

- Unilever's New Recipe for Growth (with Jordan Siegel and Barbara Zepp Larson)



- Yum! Brands (with Jordan Siegel)
- Baxter's Asia Pacific "Talent Edge" Initiative (with Jordan Siegel and Mimi Xi)

### Conferences, Consortia, and Invited Talks

2023	Non-Market Strategy Research Community Brown Bag Seminar <i>CEO Activism and Public Mobilization</i>
2023	University of Michigan <i>CEO Activism and Public Policy</i>
2023	Inspire Institute of Education and Research <i>Resource Redeployment as an Entry Advantage in Resource-Poor Settings</i>
2023	Academy of Management Annual Meeting <i>Resource Redeployment as an Entry Advantage in Resource-Poor Settings</i>
2023	Academy of Management Annual Meeting <i>Corporate Engagement in the Aftermath of the Capitol Riot</i>
2023	DRUID <i>Resource Redeployment as an Entry Advantage in Resource-Poor Settings</i>
2023	So-Cal Strategy and OT Workshop USC Marshall School of Business <i>CEO Activism and Political Participation: Experimental Evidence on Abortion Rights</i>
2022	Academy of Management Annual Meeting <i>The Value of Corporate Political Donations: Evidence from the Capitol Riot</i>
2022	Non-Market Strategy Research Community Brown Bag Seminar <i>The Value of Corporate Political Donations: Evidence from the Capitol Riot</i>
2021	Sumantra Ghoshal Strategy Conference London Business School <i>CEO Activism, Consumer Polarization, and Firm Performance</i>
2021	CCC Corporate Dynamics Brown Bag Seminar <i>CEO Activism, Consumer Polarization, and Firm Performance</i>
2021	Allied Social Science Association <i>The Impact of Mass Shootings on Gun Policy</i>
2019	APPAM Fall Research Conference <i>The Impact of Mass Shootings on Gun Policy</i>
2019	12 <sup>th</sup> Annual People & Organizations Conference Wharton, University of Pennsylvania <i>Worker Redeployment in Multi-Business Firms: An Empirical Examination</i>
2019	Georgetown University <i>The Impact of Mass Shootings on Gun Policy</i>
2019	University of Utah / BYU Winter Strategy Conference <i>Knowledge Sharing and Intra-Organizational Worker Mobility</i>
2018	SMS São Paulo, Brazil <i>The Effect of the Internet on Wages</i>

2018	Tufts University <i>The Impact of Mass Shootings on Gun Policy</i>
2017	American Society of Criminology Annual Meeting <i>The Impact of Mass Shootings on Gun Policy</i>
2017	Conference on Empirical Legal Studies Cornell Law School <i>The Impact of Mass Shootings on Gun Policy</i>
2017	TIM Doctoral Consortium Academy of Management Annual Meeting
2017	Consortium for Cooperation and Competition Wharton, University of Pennsylvania <i>The Effect of the Internet on Wages</i>
2017	Economics Experiments in the Tech Industry Stanford Institute for Economic Policy Research <i>The Effect of the Internet on Wages</i> (poster session)
2016	International Association for Conflict Management Annual Meeting <i>The Impact of Mass Shootings on Gun Policy</i>
2014	EDEN Doctoral Seminar on Advanced Strategic Management IESE Business School, Barcelona
2014	Strategic Research Initiative PhD Bootcamp IESE Business School, New York

## Work Experience

2010 – 2012	Harvard Business School, Boston, MA Research Associate for Jordan Siegel
2009 – 2010	Guaruma: Jóvenes Hondureños por el Desarrollo Educativo, La Ceiba, Honduras Assistant Director
2009	University of Pennsylvania, Philadelphia, PA Research Assistant for Jonathan Baron



# Handgun waiting periods reduce gun deaths

Michael Luca<sup>a,1</sup>, Deepak Malhotra<sup>a</sup>, and Christopher Poliquin<sup>a</sup>

<sup>a</sup>Harvard Business School, Boston, MA 02163

Edited by Philip J. Cook, Duke University, Durham, NC, and accepted by Editorial Board Member Kenneth W. Wachter September 21, 2017 (received for review December 3, 2016)

**Handgun waiting periods are laws that impose a delay between the initiation of a purchase and final acquisition of a firearm. We show that waiting periods, which create a “cooling off” period among buyers, significantly reduce the incidence of gun violence. We estimate the impact of waiting periods on gun deaths, exploiting all changes to state-level policies in the United States since 1970. We find that waiting periods reduce gun homicides by roughly 17%. We provide further support for the causal impact of waiting periods on homicides by exploiting a natural experiment resulting from a federal law in 1994 that imposed a temporary waiting period on a subset of states.**

gun policy | gun violence | waiting period | injury prevention

More than 33,000 people die in gun-related incidents each year in the United States, accounting for as many deaths as motor vehicle accidents (1). This is concerning both in absolute terms and in comparison to other developed countries, all of which have lower rates of gun violence (2). For example, if the United States could lower its firearm death rate to that of Finland (the high-income country with the second highest rate), roughly 20,000 fewer people would die from guns every year. However, there has been no meaningful reduction in the US firearm-related death rate for more than a decade. Moreover, evidence about which policies would be effective at reducing violence remains limited (3), and the types of bills that are enacted depend on the political party in power (4).

One avenue for reducing gun deaths is to draw on insights from behavioral economics and psychology, which suggest that delaying gun purchases, even for a short time, might be an effective policy tool. Visceral factors, such as anger or suicidal impulses, can spur people to inflict harm on others or themselves, but tend to be transitory states (5, 6). For example, Card and Dahl (7) find that there is a 10% increase in domestic violence following an upset loss of the local National Football League team. Moreover, behaviors triggered by such visceral states can be contrary to longer term self-interest (5, 6).

Delaying a gun purchase could create a “cooling off” period that reduces violence by postponing firearm acquisitions until after a visceral state has passed. Increasing the time it takes to acquire a gun might also close the window of opportunity for would-be perpetrators of violence to use their weapons. Finally, a mandatory delay has the potential to deter purchases among people who have malevolent, but temporary, motivations for owning a firearm.

This article explores the impact of “waiting period” laws on firearm-related homicides and suicides using 45 y of data on law changes and mortality at the state level in the United States. A waiting period is a mandatory delay between the purchase and delivery of a gun; it requires purchasers to wait, typically between 2 and 7 d, before receiving their weapons. We exploit plausibly exogenous temporal and geographic variation in waiting period laws to implement a difference-in-differences approach that identifies the causal impact of waiting periods on homicides and suicides.

We find that waiting periods cause large and statistically significant reductions in homicides. Point estimates using our full 45-y sample and all waiting period changes imply a 17% reduction in gun homicides. We provide further evidence of a causal relationship between waiting periods and lower homicide rates based on a natural experiment in which federal law imposed waiting periods on a subset of states. Estimates from this analysis

also suggest that waiting periods reduce gun homicides by 17%. The results of both analyses confirm a large and robust effect of waiting periods on homicides. We also find a negative effect of waiting periods on suicides, but the magnitude and statistical significance of the suicide effect vary across model specification.

## Data and Research Design

We construct a panel of every change to waiting period laws in the United States between 1970 and 2014, which we obtained from state statutes and session laws. We combine these changes with annual data on firearm-related deaths from the Centers for Disease Control and Prevention. Fig. 1 shows the number of states with waiting periods over time. Overall, 44 states (including the District of Columbia) have had a waiting period for at least some time between 1970 and 2014. Exploiting the significant geographic and temporal variation in the adoption of waiting periods, we implement a difference-in-differences framework to estimate the causal impact of waiting periods on gun deaths. Essentially, we compare changes in firearm-related deaths within states that adopted waiting periods with changes in firearm-related deaths in other states. We control for changing economic and demographic factors that may be correlated with higher levels of gun violence or with the decision of lawmakers to adopt policies that delay gun purchases.

To support our causal interpretation, we then restrict the analysis to the period from 1990 to 1998, during which federal policy forced many states to implement waiting periods. The Brady Handgun Violence Prevention Act (hereinafter “Brady Act”), which went into effect in February 1994, required background checks on handgun purchases from licensed firearm dealers and created a 5-d waiting period to allow sufficient time for the check. Although it was a federal policy, the Brady Act only created new waiting periods for 19 states, since some states already required a background check and waiting period, and some implemented an “instant check” system that allowed for nearly immediate background checks (thereby obviating the need for a waiting period). We provide further details regarding the Brady Act and affected states in *Identifying Policy Changes* and *Materials and Methods*.

## Significance

**Waiting period laws that delay the purchase of firearms by a few days reduce gun homicides by roughly 17%. Our results imply that the 17 states (including the District of Columbia) with waiting periods avoid roughly 750 gun homicides per year as a result of this policy. Expanding the waiting period policy to all other US states would prevent an additional 910 gun homicides per year without imposing any restrictions on who can own a gun.**

Author contributions: M.L., D.M., and C.P. designed research, performed research, analyzed data, and wrote the paper.

The authors declare no conflict of interest.

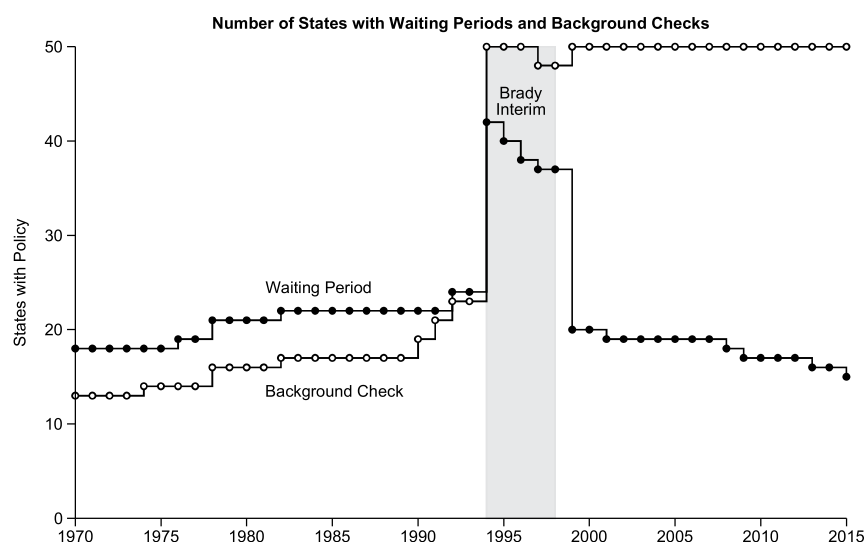
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See Commentary on page 12097.

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This article contains supporting information online at [www.pnas.org/lookup/suppl/doi:10.1073/pnas.1619896114/-DCSupplemental](http://www.pnas.org/lookup/suppl/doi:10.1073/pnas.1619896114/-DCSupplemental).



**Fig. 1.** States with handgun waiting periods and background checks on dealer sales from 1970 to 2015. Many states were required to implement these policies during the Brady interim period between February 1994 and November 1998 (shaded gray). Following prior research (8), Alabama and Ohio are coded as not requiring background checks after the Supreme Court's decision in *Printz v. United States*. Not all states had waiting periods during the Brady interim period because they implemented or already had an instant background check system that obviated the need for a waiting period to investigate gun buyers.

## Results

We begin by examining the effect of waiting periods across the full sample period from 1970 to 2014. The results of Table 1 show that waiting periods are associated with a 17% reduction in gun homicides. This effect is equivalent to ~36 fewer gun homicides

per year for a state with an average number of gun deaths. Waiting periods also lead to a 7–11% reduction in gun suicides (depending on the control variables used in the specification), which is equivalent to 22–35 fewer gun suicides per year for the average state. The results in Table 1 use a log-linear specification; we

**Table 1. Effects of handgun waiting periods and background checks on violence, 1970–2014**

Type of violence	1970–2014		1977–2014
	(1)	(2)	(3)
All homicide			
Waiting period	−0.127 (0.059)**	−0.137 (0.059)**	−0.132 (0.050)**
Background check		0.049 (0.082)	0.025 (0.081)
Gun homicide			
Waiting period	−0.188 (0.077)**	−0.187 (0.086)**	−0.186 (0.071)**
Background check		−0.004 (0.103)	0.022 (0.107)
Non-gun homicide			
Waiting period	−0.016 (0.051)	−0.048 (0.060)	−0.035 (0.037)
Background check		0.153 (0.076)**	0.036 (0.057)
All suicide			
Waiting period	−0.047 (0.021)**	−0.070 (0.023)**	−0.024 (0.011)**
Background check		0.113 (0.061)*	0.023 (0.020)
Gun suicide			
Waiting period	−0.097 (0.034)**	−0.120 (0.031)**	−0.074 (0.017)**
Background check		0.111 (0.073)	0.029 (0.028)
Non-gun suicide			
Waiting period	−0.017 (0.038)	−0.058 (0.059)	−0.006 (0.033)
Background check		0.199 (0.072)**	0.084 (0.031)**

Coefficients represent the effects of waiting periods and background checks on the natural logarithm of deaths per 100,000 adult residents. All models include state and year fixed effects. Models 1–2 include only the policy variables shown. Model 3 follows the specification of Ludwig and Cook (8) and includes alcohol consumption, poverty, income, urbanization, black population, and seven age groups. Model 3 uses fewer years of data due to missing control variables in earlier years. Summary statistics for all variables are included in Table S1. The 1970–2014 period includes 2,295 state-year observations; the model for gun homicides omits three state-years, and the model for non-gun homicides omits two state years because the death count was zero and the model is specified with a logged dependent variable. Similarly, the 1977–2014 period includes 1,938 state-years, but omits two state-years for gun homicides and one state-year for non-gun homicides. SEs, shown in parentheses, are clustered by state. Alternative model specifications presented in Tables S7 and S8 are not logged and include all state-years. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

present models with state-specific trends, models linear in the rate of violence, and Poisson models as part of [Tables S3](#) and [S5](#). The conclusion that waiting periods reduce gun homicides is robust across all specifications. The conclusion regarding suicides is robust to all specifications except those that include state-specific, linear trends ([Table S3](#)). Both conclusions are robust across models with and without controls for state-level economic and demographic changes. We also investigate the robustness of the results to the exclusion of individual states in [Fig. S1](#).

To further support the hypothesis that waiting periods lead to a reduction in gun homicides, we then focus on a natural experiment created by the Brady Act, a federal law that forced some states to adopt new waiting period and background check policies between 1994 and 1998. Ludwig and Cook (8) also use the Brady Act to study whether background checks and waiting periods affect violence. They compare “Brady states” that were subject to the Brady Act with “Brady-exempt states” that were not. However, some states that were classified as Brady states already had waiting periods and background checks before the Brady Act, and other states chose to implement an “instant” background check system instead of requiring a waiting period. As a result, the coding of Brady states in the study by Ludwig and Cook (8) fails to capture all states that had preexisting waiting periods. In contrast, we precisely code which states had waiting periods (before 1994) and which implemented waiting periods only because of the Brady Act. In total, our coding differs from theirs for 16 states. This additional accuracy allows us to assess the causal impact of waiting periods resulting from the Brady Act. The full list of differences between our coding and prior research, along with supporting citations, can be found in [Table S4](#).

We find that waiting periods led to large and statistically significant reductions in gun violence ([Table 2](#)) during the Brady interim period. Specifically, the results of column 3 of [Table 2](#) show that waiting periods implemented during the Brady interim years resulted in a 17% reduction in gun homicides. This is equivalent to roughly 39 fewer homicides per year for the average state. There was also a 6% reduction in gun suicides (i.e.,

17 fewer suicides per year for the average state). Both results are robust across models with and without controls for state-level economic and demographic changes. Notably, exploiting the Brady Act as a natural experiment produces similar estimates as the longer sample period from 1970 to 2014.

[Tables 1](#) and [2](#) also show that waiting periods have no significant effect on non-gun homicides, suggesting that people subject to waiting period laws do not substitute other means of committing homicide. This is consistent with other research (9) finding no increase in non-gun homicides in response to policies restricting access to firearms. Results for non-gun suicides, however, are less clear; some specifications suggest partial substitution toward non-gun methods of suicide in response to handgun waiting periods.

## Discussion

Our results show that waiting periods reduce gun homicides. Waiting periods for gun purchases are supported not only by the American Medical Association but also by a majority of Americans and a majority of gun owners (10, 11). Our point estimates, based on 45 y of data, suggest that the 17 states (including the District of Columbia) with waiting periods as of 2014 avoid ~750 gun homicides. Expanding the waiting period policy to states that do not currently have it would prevent an additional 910 gun homicides per year. Waiting periods would therefore reduce gun violence without imposing any restrictions on who can own a gun.

## Materials and Methods

Our main specifications are of the form:

$$r_{it} = \alpha_i + \lambda_t + \beta W_{it} + \gamma B_{it} + \delta' X_{it} + \epsilon_{it},$$

where  $r_{it}$  is the natural logarithm of the rate of violence (homicides or suicides) per 100,000 adult residents,  $W_{it}$  is an indicator for handgun waiting periods and  $B_{it}$  is an indicator for whether background checks are required for dealer handgun sales. We include an indicator variable for background checks on handgun purchases from licensed firearm dealers because a major source of policy variation in our dataset (the Brady Act) also affected

**Table 2. Effects of handgun waiting periods and background checks on violence, 1990–1998**

Type of violence	Brady period, 1990–1998		
	(1)	(2)	(3)
All homicide			
Waiting period	−0.073 (0.084)	−0.130 (0.077)*	−0.145 (0.060)**
Background check		0.091 (0.064)	0.010 (0.053)
Gun homicide			
Waiting period	−0.103 (0.093)	−0.179 (0.087)**	−0.181 (0.068)**
Background check		0.120 (0.080)	0.033 (0.065)
Non-gun homicide			
Waiting period	−0.019 (0.068)	−0.035 (0.064)	−0.072 (0.050)
Background check		0.025 (0.044)	−0.043 (0.039)
All suicide			
Waiting period	−0.016 (0.021)	−0.022 (0.023)	−0.036 (0.020)*
Background check		0.009 (0.022)	−0.007 (0.019)
Gun suicide			
Waiting period	−0.039 (0.024)	−0.053 (0.028)*	−0.066 (0.021)***
Background check		0.023 (0.028)	−0.003 (0.024)
Non-gun suicide			
Waiting period	0.050 (0.021)**	0.035 (0.022)	0.018 (0.022)
Background check		0.024 (0.023)	0.009 (0.018)

Coefficients represent the effects of waiting periods and background checks on the natural logarithm of deaths per 100,000 adult residents. All models include state and year fixed effects. Models 1–2 include only the policy variables shown. Model 3 follows the specification of Ludwig and Cook (8) and includes alcohol consumption, poverty, income, urbanization, black population, and seven age groups. Summary statistics for all variables are included in [Table S2](#). The sample includes 459 state-year observations for all models. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .



background check policies. As seen in Tables 1 and 2, the estimated impact of background checks depends on model specification. We also incorporate time-varying state-level control variables that may influence rates of gun violence (8),  $X_{it}$ , including alcohol consumption, poverty, income, urbanization, black population, and seven age groups. Summary statistics for these variables are included in Tables S1 and S2. The  $\alpha_i$  and  $\lambda_t$  parameters represent state and year fixed effects. These fixed effects control for stable, state-specific factors affecting violence and time-varying factors that affect all states identically. It is impossible to control for all time-varying, state-specific factors that affect gun violence. For example, policing tactics, drug use, and environmental factors such as lead exposure might not have changed uniformly across states over time and may also affect violence. However, the consistency between our estimates during the short (Brady interim) period and the longer period (including all waiting period changes since 1970) supports our interpretation of the results. The model parameters are estimated via least squares weighted by state population. We then calculate the percentage effect of waiting periods on violence using the estimator described by Kennedy (12).

We code a state as having a waiting period if it imposes any mandatory delay on the purchase of a handgun or has a permitting system for dealer and private sales. (In Table S5, we estimate models with a separate control variable for handgun permit systems and show that the effect of waiting periods is not limited to states with permitting systems.) Currently, 10 states and the District of Columbia impose an explicit waiting period on handgun

sales, and an additional five states have permitting systems for private and dealer sales that result in a delay of firearm purchases. Forty-four states have had a handgun waiting period at some point since 1970, although 19 implemented the policy only due to the Brady Act's interim provisions, in effect from February 1994 to November 1998. These provisions required local law enforcement agencies to conduct background checks on handgun purchases from licensed firearm dealers and required a 5-d waiting period to conduct the check. Some states already required background checks and/or waiting periods before the Brady Act, and were therefore not affected by the new law, but other states were forced to adopt a new waiting period due to the federal policy change. When the permanent provisions of the Brady Act took effect on November 30, 1998, the federal waiting period requirement was replaced with an instant background check system [the National Instant Criminal Background Check System (NICS)]. As a result, many states discarded their waiting periods after 1998 because the NICS eliminated the need for a waiting period to investigate purchasers' backgrounds. We use the subset of waiting period changes that resulted from the Brady Act as a natural experiment to provide further support for our analysis of the full sample period from 1970 to 2014.

Although nine states have also had a waiting period on long-guns (i.e., rifles and shotguns) sometime since 1970, we focus on handgun waiting periods because handguns account for 70–80% of firearm homicides (13) and because a major source of variation in our data, the Brady Act's interim period, only affected handgun sales.

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# Supporting Information

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## Summary Statistics

Tables S1 and S2 provide summary statistics for variables used in the main analyses. Table S1 shows summary statistics for variables used for analyses presented in Table 1, covering the full 1970–2014 sample period. Table S2 shows summary statistics for variables used for the analysis of the Brady interim period in Table 2, covering 1990–1998.

## Identifying Policy Changes

In our first set of analyses, covering 1970–2014, we extend prior coding of policy changes by including an additional 36 y of data with 25 changes in waiting period policies. Our approach to identifying changes in waiting period policies also improves on Ludwig and Cook's (8) classification of states affected by the Brady Handgun Violence Prevention Act. Prior research coded all states subject to the Brady Act's interim provisions as treatment states, but some of these states already had background checks and/or waiting periods before the interim period. Table S4 details the differences between our coding and that of Ludwig and Cook (8). In total, our coding differs for 16 states; the table footnotes provide supporting citations for each difference. We find that these improvements more accurately measure the effects of waiting periods on homicides, which we now find to be robust and statistically significant at conventional levels, even when we restrict the sample to the same years examined in prior research.

## Robustness: State-Specific Trends

If states that do and do not adopt waiting periods have different trends in violence before the implementation of the waiting period, then one might be concerned that our results reflect these different trends rather than the impact of the waiting period policy. To allow for the possibility of differential secular trends, Table S3 estimates a log-linear model with linear trends that vary by state for the 1970–2014 time period [We do not estimate models with state-specific trends for the analysis of the Brady interim period (1990–1998) because there is too little pretreatment data to identify preexisting, state-specific trends in gun violence (14)]. This model produces similar estimates for the effect of waiting periods on homicides, suggesting that differential trends are not the main driver of the results and providing further support for our interpretation. The results for suicides, however, differ across specification and are not robust to the inclusion of control variables and state-specific trends in suicide. The model without trends in column 3 of Table 1 suggests that waiting periods reduce gun suicides by 7%, while the model in column 3 of Table S3 suggests no reduction. The results of Table S3 also suggest that any decrease in gun suicides due to waiting periods is offset by an increase in non-gun suicides.

## Robustness: Falsification Exercise and Dynamic Effects

To shed further light on the dynamics of the effects shown in Table 1, Table S6 reestimates the model in column 3 of Table 1, but includes leads and lags of the policy change, specifically including indicator variables for the years before and after implementation of a waiting period. We find that the impact of

waiting periods does not appear until the waiting period has been adopted, providing further support for our causal interpretation. Violence appears to fall soon after implementation, although the single-year estimates are imprecise.

## Robustness: Other Changes in Gun Policy

While the results overall point to the causal effect of waiting periods, one might still be concerned that other gun policy changes are correlated with the timing of waiting period changes. To address this concern, we provide evidence that the effects reported in Table 1 are robust to the inclusion of controls for other gun policies in a state. Specifically, in Table S5, we reestimate the models of columns 2 and 3 in Table 1, but include additional variables for handgun permit and concealed carry policies to account for potential correlation between the implementation of these policies and waiting periods. The results in Table S5 show that the inclusion of other gun policies in the model does not change our conclusion that waiting periods reduce gun homicides and suicides. Our study uses a natural experiment embedded in the Brady Act to identify the impact of waiting periods; estimating the causal impact of exogenous changes to other gun policies is beyond the scope of this study. Other research focuses on the impact of handgun permits (15, 16) and concealed carry laws (17–20).

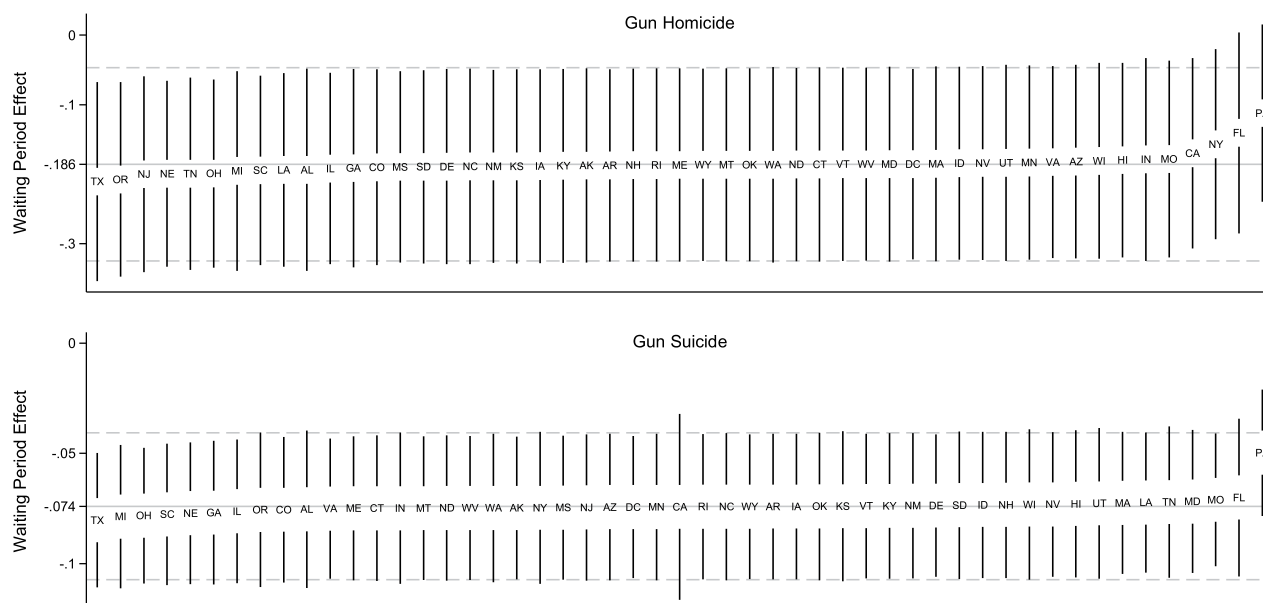
## Alternative Model Specifications

Alternative specifications for the effect of waiting periods on homicides and suicides produce similar point estimates (Tables S7 and S8). The estimates in Table S7 are based on models linear in the rate of violence. The results in columns 2 and 3 imply that waiting periods reduce gun homicides by roughly 18% and gun suicides by 5–9% for a state with an average rate of violence. Results for the Poisson model (Table S8) imply reductions of 18–20% and 7–11.6% for gun homicides and suicides, respectively, while estimates based on the log-linear model presented in the main text and Table 1 imply 17% and 7–11% reductions.

Additionally, we examine unweighted, least-squares estimates (Tables S9 and S10). The coefficient estimates on the waiting period dummy from the unweighted regressions are attenuated relative to the weighted results. This suggests that the effect of waiting period policies is heterogeneous, with larger states experiencing greater reductions in violence than smaller states (21). To ensure our results are not driven by outlier states, we reestimate the model of gun homicide and suicide rates (column 3 of Table 1), but exclude one state at a time. Fig. S1 shows the 51 resulting coefficients (one from excluding each state and the District of Columbia) for homicides and suicides. The coefficient estimates are consistently negative. As expected from the difference between the weighted and unweighted estimates, large states like Pennsylvania and Florida seem to exert downward pressure on the coefficient.

## Complete Coefficient Estimates

Table S11 presents coefficient estimates for all variables included in model 3 of Table 1. This model uses the same control variables as prior research by Ludwig and Cook (8).



**Fig. S1.** Estimates of the effect of waiting periods on gun homicides and suicides, dropping each state individually from the analysis and reestimating model 3 of Table 1. Bars are  $1.96 \pm \text{SE}$  of the waiting period coefficient. Solid lines mark the full sample estimates, and dashed lines are  $1.96 \pm$  full sample SE.

**Table S1. State-level summary statistics: 1970–2014 (Table 1)**

Variable	Mean	SD	p5	p10	p50	p90	p95
<b>Years 1970–2014</b>							
Gun homicide rate	5.7	4.9	1.0	1.4	4.4	11.1	14.4
Homicide rate	8.5	6.7	2.1	2.6	6.8	15.6	19.4
Gun suicide rate	10.2	4.2	3.1	4.0	10.2	15.0	17.1
Suicide rate	17.3	4.7	10.2	11.9	16.7	23.7	26.0
Handgun waiting period	0.45	0.49	0	0	0	1	1
Background checks	0.64	0.48	0	0	1	1	1
<b>Years 1977–2014 (Control variables for model 3)</b>							
Alcohol consumption	2.9	0.8	2.0	2.1	2.7	3.8	4.3
Income per capita	25.4	5.8	17.2	18.6	24.8	32.6	35.7
<b>Demographics, %</b>							
Poverty	13.1	4.0	7.9	8.7	12.5	18.5	20.9
Urban areas	64.2	20.1	29.5	35.5	64.9	89.0	91.9
Black	11.2	11.8	0.4	0.7	7.4	27.5	32.1
Ages 0–14 y	21.4	2.4	17.9	18.7	21.3	24.3	25.8
Ages 15–17 y	4.5	0.6	3.7	3.9	4.4	5.5	5.8
Ages 18–24 y	10.9	1.6	8.9	9.2	10.3	13.4	13.8
Ages 25–34 y	15.1	2.2	12.0	12.5	15.0	17.9	18.7
Ages 35–44 y	14.0	1.9	10.8	11.3	14.1	16.3	16.9
Ages 45–54 y	12.0	2.2	9.0	9.3	12.0	14.9	15.4
Ages 55–64 y	9.7	1.7	7.6	7.9	9.2	12.3	12.9

Homicide and suicide rates are adult (21+) deaths per 100,000 adult residents. Alcohol consumption is measured in gallons of ethanol per capita, and income is measured in thousands of 1998 dollars. Demographic control variables are percentages of total state population. Columns beginning with “p” represent percentiles of the distribution; for example, “p10” means the 10th percentile.



**Table S2. State-level summary statistics: 1990–1998 (Table 2)**

Variable	Mean	SD	p5	p10	p50	p90	p95
Gun homicide rate	5.9	6.1	1.0	1.4	4.6	10.4	12.3
Homicide rate	8.8	8.0	2.1	2.7	7.0	14.7	18.3
Gun suicide rate	10.2	4.0	3.2	4.0	10.6	15.0	17.3
Suicide rate	16.6	4.5	9.6	11.4	16.1	22.7	24.9
Handgun waiting period	0.63	0.47	0	0	1	1	1
Background checks	0.74	0.43	0	0	1	1	1
Alcohol consumption	2.6	0.6	2.0	2.1	2.6	3.1	4.1
Income per capita	24.3	3.8	19.1	19.9	23.9	29.1	31.7
Demographics, %							
Poverty	13.3	4.0	8.2	8.9	12.5	19.0	21.1
Urban areas	63.5	19.9	29.0	35.1	63.9	87.5	91.1
Black	11.1	12.0	0.4	0.5	7.3	27.5	31.9
Ages 0–14 y	21.9	1.9	19.4	20.0	21.6	24.1	25.2
Ages 15–17 y	4.3	0.5	3.5	3.7	4.2	4.9	5.1
Ages 18–24 y	9.9	0.9	8.4	8.9	9.9	11.0	11.6
Ages 25–34 y	15.7	1.6	13.1	13.7	15.6	17.7	18.4
Ages 35–44 y	16.0	1.0	14.4	14.8	15.9	17.1	17.7
Ages 45–54 y	11.5	1.2	9.7	10.0	11.5	13.1	13.5
Ages 55–64 y	8.2	0.7	7.1	7.5	8.2	8.9	9.1

Homicide and suicide rates are adult (21+) deaths per 100,000 adult residents. Alcohol consumption is measured in gallons of ethanol per capita; income is thousands of 1998 dollars. Demographic control variables are percentages of total state population. Columns beginning with “p” represent percentiles of the distribution; for example, “p10” means the 10th percentile.

**Table S3. States that implemented background checks and waiting periods during the Brady Act's interim period from February 1994 through November 1998, according to Ludwig and Cook (8) and this study**

State	Ludwig and Cook (8)		New coding (this study)	
	Background check	Waiting period	Background check	Waiting period
<b>Alabama*</b>	■	■	■	■
Alaska	■	■	■	■
<b>Arizona†</b>	■	■	■	■ Feb-Oct 1994
Arkansas	□	□	□ Feb 1994-June 1997	□ Feb 1994-June 1997
California				
Colorado	■		■	
Connecticut				
Delaware				
District of Columbia				
Florida				
<b>Georgia‡</b>	■	■	■	□ Feb 1994-Dec 1995
Hawaii				
<b>Idaho§</b>	■	■	■	□ Feb-May 1994
Illinois				
Indiana				
Iowa				
Kansas	■	■	■	■
Kentucky	■	■	■	■
Louisiana	■	■	■	■
Maine	■	■	■	■
Maryland				
Massachusetts				
Michigan				
<b>Minnesota¶</b>	■			
Mississippi	■	■	■	■
Missouri				
Montana	■	■	■	■
<b>Nebraska#</b>	■	■		
<b>Nevada<sup>  </sup></b>				
<b>New Hampshire**</b>	■	■	■	□ Feb-Dec 1994
New Jersey				
New Mexico	■	■	■	■
New York				
<b>North Carolina††</b>	■	■		
North Dakota	■	■		■
<b>Ohio‡‡</b>	□	□	□ Feb 1994-June 1997	
Oklahoma	■	■		■
Oregon				
<b>Pennsylvania§§</b>	■	■	■	
<b>Rhode Island¶¶</b>	■	■	■	
<b>South Carolina##</b>	■	■	■	
<b>South Dakota   </b>	■	■	■	
<b>Tennessee***</b>	■	■		
Texas	■	■	■	■
Utah	■	■	■	■
Vermont	■	■	■	■
Virginia				
<b>Washington†††</b>	■	■	■	■
West Virginia	■	■	■	■
Wisconsin				
Wyoming	■	■	■	■

The coding of states in boldface differs; an explanation of differences is provided in table footnotes. Dates are noted for cases in which policies changed during the interim period. ■, state got policy for full interim period; □, state got policy for part of interim period.

\*Alabama had a 2-d waiting period on handgun purchases before implementation of the Brady Act (Code of Ala. § 13A-11-77).

†Arizona created an instant check background system in October 1994, and therefore had effectively no waiting period for most of the Brady Act's interim period (Ariz. Rev. Stat. Ann. § 13-3114).

‡Georgia implemented an instant check system in January 1996 (Ga. Code Ann. § 16-11-170).

§Idaho implemented an instant check system in June 1994 (Ida. Code § 19-5403).

¶Minnesota created a permit system in 1977 that required background checks and a 7-d waiting period for handgun purchases (Minn. Stat. § 62A.7131 et seq.).

#Nebraska was exempt from the Brady Act (22, 23). Furthermore, it created a handgun permit system with a background check and 2-d waiting period in 1991 (Neb. Rev. Stat. § 69-2404 et seq.).

||Ludwig and Cook (8) say Nevada was classified as a control state because it's pre-Brady Act laws were strict enough to warrant an exemption even though it was subject to the Brady Act. We cannot find evidence of this; Nevada had neither a background check nor waiting period requirement before implementation of the Brady Act (24) and was subject to the act's provisions (23). We classify the state as not having a waiting period because the state implemented an instant check system (25).

\*\*New Hampshire implemented an instant check system in January 1995 (N.H. Rev. Stat. Ann. § 159-C).

††We classify North Carolina as a control state because it implemented a handgun permit system in 1919 (N.C. Gen. Stat. § 14-402 et seq.). An explicit background check requirement was not added to the statutes until 1995, but the law previously required superior court clerks to certify that handgun permit applicants were of "good moral character" and included felonies, indictments, fugitive status, and mentally ill persons among those not of such character (N.C. Gen. Stat. § 14-404).

‡‡Ohio was subject to the Brady Act's interim provisions (22, 23) but had instant background checks (25), and is therefore coded as not implementing a waiting period. Like Ludwig and Cook (8), we code Ohio as stopping background checks after the Supreme Court's decision in *Printz v. United States* in June of 1997. We cannot find a statute or executive order for Ohio, and therefore rely exclusively on federal government reports (22, 23, 25).

§§Pennsylvania already had a 2-d waiting period before implementation of the Brady Act (24). We therefore code the state as only implementing the Brady Act's background check provisions. The state abandoned its waiting period in 1998 when instant checks became available (text and legislative history of 18 Pa.C.S.A. § 6111).

¶¶Rhode Island was subject to the Brady Act despite requiring both a background check and waiting period as part of its handgun permit process before 1994 (24). It therefore did not newly implement background checks or waiting periods as a result of the Brady Act (R.I. Gen. Laws § 11-47-35 et seq.).

##South Carolina's Law Enforcement Division ran an instant check system at the time the Brady Act was implemented (22, 25, 26), and is therefore coded as not implementing a waiting period. South Carolina's governor created the instant check system by executive order (26).

|||South Dakota had a 2-d waiting period before implementation of the Brady Act (since at least 1935) that was not repealed until 2009 (S.D. Codified Laws § 23-7-9).

\*\*\*Tennessee was subject to the Brady Act even though it already required a background check and 15-d waiting period (24) (Tenn. Code Ann. § 39-17-1316). It is therefore coded as not newly implementing these laws due to the Brady Act's interim provisions.

†††Washington had background checks before the Brady Act but was not Brady-exempt because it did not require the chief law enforcement officer in the area where the purchaser lived to conduct the check (Wash. Rev. Code Ann. § 9A.1.090).

**Table S4. Effects of handgun waiting periods and background checks on violence, including state-specific trends, 1970–2014**

Type of violence	1970–2014		1977–2014
	(1)	(2)	(3)
All homicide			
Waiting period	−0.118 (0.049)**	−0.129 (0.049)**	−0.086 (0.045)*
Background check		0.033 (0.057)	0.001 (0.047)
Gun homicide			
Waiting period	−0.181 (0.066)***	−0.195 (0.071)***	−0.124 (0.050)**
Background check		0.043 (0.077)	0.014 (0.068)
Non-gun homicide			
Waiting period	−0.011 (0.039)	−0.014 (0.038)	−0.030 (0.047)
Background check		0.011 (0.051)	−0.015 (0.035)
All suicide			
Waiting period	0.015 (0.013)	0.017 (0.013)	0.022 (0.016)
Background check		−0.005 (0.017)	−0.006 (0.015)
Gun suicide			
Waiting period	−0.044 (0.017)**	−0.045 (0.020)**	−0.012 (0.016)
Background check		0.002 (0.018)	−0.017 (0.017)
Non-gun suicide			
Waiting period	0.056 (0.019)***	0.050 (0.020)**	0.048 (0.024)*
Background check		0.020 (0.022)	0.019 (0.024)

Coefficients represent the effects of waiting periods and background checks on the natural logarithm of deaths per 100,000 adult residents. Models mirror Table 1, but include a state-specific, linear trend in addition to state and year fixed effects. Models 1–2 include only the policy variables shown. Model 3 follows the specification of Ludwig and Cook (8) and uses fewer years of data due to missing control variables in earlier years. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

**Table S5. Effect of handgun waiting periods relative to adoption year, 1977–2013**

	Homicides			Suicides		
	All	Gun	Non-gun	All	Gun	Non-gun
Time relative to waiting period	(1)	(2)	(3)	(4)	(5)	(6)
2 y before	−0.024 (0.047)	−0.038 (0.056)	0.004 (0.060)	0.015 (0.021)	0.001 (0.024)	0.045 (0.031)
1 y before	−0.053 (0.051)	−0.076 (0.060)	−0.014 (0.052)	0.025 (0.017)	0.003 (0.018)	0.046 (0.029)
Adoption year	−0.087 (0.054)	−0.106 (0.077)	−0.063 (0.051)	0.008 (0.021)	−0.014 (0.026)	0.006 (0.034)
1 y after	−0.147 (0.060)**	−0.178 (0.080)**	−0.11 (0.065)*	−0.032 (0.022)	−0.082 (0.026)***	−0.016 (0.032)
2 y after	−0.147 (0.058)**	−0.176 (0.082)**	−0.086 (0.043)*	−0.004 (0.016)	−0.061 (0.023)***	0.039 (0.030)
3 y after	−0.145 (0.060)**	−0.198 (0.083)**	−0.048 (0.053)	−0.007 (0.017)	−0.063 (0.022)***	0.04 (0.034)
4+ y after	−0.129 (0.053)**	−0.188 (0.072)**	−0.021 (0.041)	−0.022 (0.012)*	−0.071 (0.016)***	−0.006 (0.037)

Models mirror column 3 of Table 1, but include an indicator variable for years before and after implementation of the waiting period \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

**Table S6. Estimates of the waiting period effect, controlling for other gun policies**

Type of violence	1970–2014	1977–2014
	(1)	(2)
All homicide		
Waiting period	−0.141 (0.061)**	−0.137 (0.051)**
Background check	0.054 (0.065)	0.019 (0.068)
Handgun permit	0.021 (0.089)	0.051 (0.091)
Shall-issue CCW	0.002 (0.104)	0.056 (0.095)
May-issue CCW	0.006 (0.118)	0.062 (0.097)
Gun homicide		
Waiting period	−0.201 (0.086)**	−0.194 (0.074)**
Background check	0.010 (0.084)	0.007 (0.090)
Handgun permit	0.075 (0.093)	0.084 (0.125)
Shall-issue CCW	−0.019 (0.119)	0.078 (0.118)
May-issue CCW	−0.035 (0.137)	0.046 (0.118)
Non-gun homicide		
Waiting period	−0.033 (0.055)	−0.035 (0.033)
Background check	0.135 (0.055)**	0.042 (0.053)
Handgun permit	−0.077 (0.100)	0.006 (0.054)
Shall-issue CCW	0.063 (0.083)	0.045 (0.062)
May-issue CCW	0.110 (0.096)	0.118 (0.073)
All Suicide		
Waiting period	−0.037 (0.023)	−0.016 (0.011)
Background check	0.066 (0.029)**	0.012 (0.017)
Handgun Permit	−0.167 (0.070)**	−0.092 (0.036)**
Shall-issue CCW	0.044 (0.040)	0.013 (0.026)
May-issue CCW	0.025 (0.046)	0.014 (0.026)
Gun suicide		
Waiting period	−0.083 (0.031)***	−0.066 (0.019)***
Background check	0.064 (0.034)*	0.015 (0.023)
Handgun permit	−0.196 (0.078)**	−0.101 (0.037)***
Shall-issue CCW	0.007 (0.048)	0.008 (0.031)
May-issue CCW	−0.029 (0.063)	−0.012 (0.039)
Non-gun suicide		
Waiting period	−0.021 (0.047)	−0.001 (0.030)
Background check	0.119 (0.050)**	0.062 (0.030)**
Handgun permit	−0.156 (0.062)**	−0.059 (0.040)
Shall-issue CCW	0.176 (0.049)***	0.085 (0.028)***
May-issue CCW	0.152 (0.054)***	0.093 (0.027)***

Coefficients estimate the effect of waiting periods and background checks on the number of deaths per 100,000 adult residents. Models mirror those of Table 1. Model 1 includes only the policy variables shown. Model 2 follows the specification of Ludwig and Cook (8) and uses fewer years of data due to missing control variables in earlier years. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ . CCW, carrying of a concealed weapon.

**Table S7. Alternative specifications for the effect of handgun waiting periods and background checks on violence from 1970 to 2014: Linear rate**

Type of violence	1970–2014	1977–2014	
	(1)	(2)	(3)
All homicide			
Waiting period	–1.372 (0.772)*	–1.332 (0.790)*	–1.138 (0.477)**
Background check		–0.190 (1.046)	–0.412 (0.960)
Gun homicide			
Waiting period	–1.185 (0.627)*	–1.054 (0.686)	–1.010 (0.412)**
Background check		–0.627 (0.806)	–0.398 (0.791)
Non-gun homicide			
Waiting period	–0.187 (0.186)	–0.278 (0.191)	–0.129 (0.131)
Background check		0.436 (0.324)	–0.014 (0.219)
All suicide			
Waiting period	–0.906 (0.325)***	–1.238 (0.391)***	–0.459 (0.167)***
Background check		1.600 (1.157)	0.070 (0.328)
Gun suicide			
Waiting period	–0.882 (0.277)***	–0.912 (0.327)***	–0.533 (0.203)**
Background check		0.143 (0.669)	–0.453 (0.338)
Non-gun suicide			
Waiting period	–0.024 (0.222)	–0.326 (0.357)	0.073 (0.174)
Background check		1.458 (0.615)**	0.524 (0.189)***

Coefficients estimate the effect of waiting periods and background checks on the number of deaths per 100,000 adult residents. All models include state and year fixed effects and mirror those of Table 1. Model 3 uses fewer years of data due to missing control variables in earlier years. The analysis covering 1970–2014 includes 2,295 state-years; the analysis with control variables covering 1977–2014 includes 1,938 state-years. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

**Table S8. Alternative specifications for the effect of handgun waiting periods and background checks on violence from 1970 to 2014: Poisson**

Type of violence	1970–2014	1977–2014	
	(1)	(2)	(3)
All homicide			
Waiting period	–0.153 (0.049)***	–0.155 (0.050)***	–0.125 (0.051)**
Background check		0.007 (0.076)	–0.002 (0.084)
Gun homicide			
Waiting period	–0.209 (0.064)***	–0.198 (0.072)***	–0.177 (0.074)**
Background check		–0.039 (0.094)	–0.007 (0.112)
Non-gun homicide			
Waiting period	–0.031 (0.046)	–0.060 (0.050)	–0.012 (0.036)
Background check		0.100 (0.072)	0.001 (0.055)
All suicide			
Waiting period	–0.047 (0.019)**	–0.076 (0.023)***	–0.032 (0.010)***
Background check		0.127 (0.070)*	0.032 (0.021)
Gun suicide			
Waiting period	–0.089 (0.026)***	–0.116 (0.030)***	–0.075 (0.017)***
Background check		0.111 (0.075)	0.032 (0.030)
Non-gun suicide			
Waiting period	–0.010 (0.031)	–0.053 (0.053)	0.001 (0.032)
Background check		0.207 (0.078)***	0.088 (0.031)***

Coefficients are based on a Poisson model for the count of deaths using adult population as the exposure variable. All models include state and year fixed effects and mirror those of Table 1. Model 3 uses fewer years of data due to missing control variables in earlier years. The analysis covering 1970–2014 includes 2,295 state-years; the analysis with control variables covering 1977–2014 includes 1,938 state-years. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

**Table S9. Unweighted estimates of the effects of handgun waiting periods and background checks on violence: Full sample period**

Type of violence	1970–2014		1977–2014
	(1)	(2)	(3)
All homicide			
Waiting period	–0.007 (0.050)	–0.012 (0.052)	–0.047 (0.051)
Background check		0.018 (0.047)	0.022 (0.050)
Gun homicide			
Waiting period	–0.042 (0.060)	–0.029 (0.066)	–0.067 (0.066)
Background check		–0.049 (0.068)	0.011 (0.068)
Non-gun homicide			
Waiting period	0.055 (0.049)	0.020 (0.053)	–0.003 (0.044)
Background check		0.134 (0.049)***	0.039 (0.047)
All suicide			
Waiting period	–0.020 (0.017)	–0.045 (0.017)**	–0.028 (0.012)**
Background check		0.097 (0.029)***	0.032 (0.018)*
Gun suicide			
Waiting period	–0.044 (0.023)*	–0.070 (0.021)***	–0.063 (0.018)***
Background check		0.098 (0.032)***	0.051 (0.023)**
Non-gun suicide			
Waiting period	–0.016 (0.034)	–0.064 (0.041)	–0.029 (0.029)
Background check		0.186 (0.044)***	0.087 (0.032)***

This table mirrors Table 1, but models are not population-weighted. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

**Table S10. Unweighted estimates of the effects of handgun waiting periods and background checks on violence: Brady period**

Type of violence	Brady period, 1990–1998		
	(1)	(2)	(3)
All homicide			
Waiting period	–0.047 (0.033)	–0.048 (0.035)	–0.012 (0.040)
Background check		0.003 (0.035)	–0.019 (0.043)
Gun homicide			
Waiting period	–0.081 (0.044)*	–0.070 (0.048)	–0.015 (0.051)
Background check		–0.032 (0.053)	–0.045 (0.065)
Non-gun homicide			
Waiting period	0.005 (0.034)	–0.006 (0.039)	0.009 (0.039)
Background check		0.033 (0.037)	–0.012 (0.038)
All suicide			
Waiting period	0.018 (0.016)	0.023 (0.017)	0.008 (0.017)
Background check		–0.014 (0.022)	0.000 (0.014)
Gun suicide			
Waiting period	–0.019 (0.019)	–0.019 (0.023)	–0.010 (0.019)
Background check		–0.000 (0.026)	–0.017 (0.017)
Non-gun suicide			
Waiting period	0.040 (0.019)**	0.035 (0.020)*	0.015 (0.022)
Background check		0.013 (0.024)	0.036 (0.023)

This table mirrors Table 1, but models are not population-weighted. \* $P < 0.10$ ; \*\* $P < 0.05$ .

Table S11. Effects of handgun waiting periods on violence, 1970–2014

Variable	Homicides			Suicides		
	All	Gun	Non-gun	All	Gun	Non-gun
	(1)	(2)	(3)	(4)	(5)	(6)
Waiting period	−0.132** (0.050)	−0.186** (0.071)	−0.035 (0.037)	−0.024** (0.011)	−0.074*** (0.017)	−0.006 (0.033)
Background check	0.025 (0.081)	0.022 (0.107)	0.036 (0.057)	0.023 (0.020)	0.029 (0.028)	0.084** (0.031)
Alcohol consumption	0.155** (0.065)	0.142* (0.075)	0.198*** (0.071)	0.144*** (0.039)	0.147*** (0.045)	0.128*** (0.045)
Poverty	−0.004 (0.006)	−0.006 (0.007)	−0.003 (0.005)	0.001 (0.002)	0.002 (0.002)	−0.005 (0.004)
Income	−0.002 (0.011)	0.003 (0.013)	−0.003 (0.011)	−0.009*** (0.003)	−0.011** (0.004)	−0.021*** (0.005)
Urban	0.002 (0.006)	0.001 (0.007)	0.003 (0.006)	0.003 (0.003)	0.002 (0.003)	0.009** (0.004)
Black	0.035* (0.020)	0.040* (0.023)	0.022 (0.016)	0.004 (0.009)	0.024* (0.012)	−0.011 (0.010)
Age under 14 y	0.033 (0.038)	0.057 (0.055)	0.005 (0.027)	−0.003 (0.015)	0.002 (0.017)	0.013 (0.021)
Age 15–17 y	−0.136** (0.062)	−0.106 (0.077)	−0.145* (0.073)	−0.084** (0.035)	−0.171*** (0.040)	−0.068 (0.052)
Age 18–24 y	0.015 (0.046)	0.017 (0.061)	0.014 (0.047)	0.002 (0.020)	0.037* (0.021)	0.010 (0.025)
Age 25–34 y	−0.035 (0.034)	−0.038 (0.045)	−0.015 (0.029)	0.016 (0.019)	0.013 (0.022)	0.041 (0.026)
Age 35–44 y	−0.008 (0.051)	−0.038 (0.063)	0.044 (0.047)	−0.009 (0.017)	0.005 (0.023)	0.024 (0.023)
Age 45–54 y	0.056 (0.034)	0.107** (0.046)	0.009 (0.029)	0.037** (0.016)	0.027 (0.020)	0.016 (0.028)
Age 55–64 y	0.029 (0.061)	−0.025 (0.085)	0.126*** (0.044)	0.020 (0.022)	0.022 (0.033)	0.090** (0.036)
Observations	1,938	1,936	1,937	1,938	1,938	1,938
Adjusted $R^2$	0.91	0.90	0.85	0.92	0.97	0.84

This table reports coefficients for all variables included in model 3 of Table 1. The dependent variable is the natural logarithm of adult deaths (21+) per 100,000 adult residents. The observation count for gun homicides is two less than the full sample count because North Dakota had no adult gun homicides in 2008 and Vermont had no adult gun homicides in 2009. The observation count for non-gun homicides is one less than the full sample count because North Dakota had no adult non-gun homicides in 2003. All models include state and year fixed effects. SEs, shown in parentheses, are clustered by state. \* $P < 0.10$ ; \*\* $P < 0.05$ ; \*\*\* $P < 0.01$ .

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EVIDENCE FROM HANDGUN PURCHASE DELAY LAWS

John J. Donohue  
Samuel V. Cai  
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John Donohue has at various times served as an expert witness in litigation involving firearm regulation. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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Age and Suicide Impulsivity: Evidence from Handgun Purchase Delay Laws

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**ABSTRACT**

We provide the first quasi-experimental estimates of variation in suicide impulsivity by age by examining the impact of firearm purchase delay laws by age. Prior studies of firearm purchase delay laws use traditional two-way-fixed-effects estimation, but we demonstrate that bias due to heterogeneous treatment effects may have inflated previous estimates relative to our stacked-regression approach. We also develop a triple-difference stacked-regression estimator to confirm the robustness of our results. We find that purchase delay laws reduce firearm suicide for the overall adult population, but this effect is largely driven by a 6.1 percent reduction in firearm suicides for young adults ages 21-34. We demonstrate that the relationship between purchase delay laws and firearm suicide reduction weakens with age and is not driven by gun ownership rates. We argue that this is due to the impulsiveness of young adults in committing suicide, indicating that removing firearm access for young adults may provide a critical deterrent to suicide.

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# Age and Suicide Impulsivity: Evidence from Handgun Purchase Delay Laws

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## 1 Introduction

In 2021, about 12.3 million American adults had serious suicidal ideation, and 48,000 people died by suicide (CDC, 2023). Given the gravity and prevalence of deaths by suicide, preventing suicide is an increasingly important policy priority and research interest. 988, the Department of Health and Human Services’s new Suicide and Crisis Lifeline launched in 2022, has received nearly a billion dollars in federal funding alone (HHS, 2023). Economists have identified important predictors of suicide, such as social cohesion (Becker and Woessman, 2018), income inequality (Daly, Wilson and Johnson, 2013), and unemployment (Breuer, 2014), highlighting the impact of economic and social policy on suicidality. Moreover, economic research has directly measured the beneficial effects of particular policies on suicide rates, such as cash transfers (Christian, Hensel and Roth, 2019), unilateral divorce (Stevenson and Wolfers, 2006), and required mental health benefits as part of health insurance coverage (Lang, 2013). One significant challenge in translating these research findings to policy, however, lies in the fact that much of the economic research studies the effects of socio-economic phenomena and policies on a large and diverse group of adults. As such, the overall findings in these studies may mask substantial heterogeneous effects within different subpopulations. In particular, descriptive evidence indicates that patterns and circumstances of suicide vary substantially across age, suggesting that the effect sizes of various suicide prevention policies may also vary by age (McLone et al., 2016).

One key difference in risk for suicide between younger and older adults is the difference in impulsivity<sup>1</sup> between these two groups. Psychologists and public health researchers have posited several linkages between impulsive tendencies and suicidal behavior, with some attributing suicide

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<sup>1</sup>While there exist many measures of impulsivity (McCullumsmith et al., 2014), we define an impulsive suicide as one that could be prevented by a delay in access to a chosen suicide mechanism, namely firearms. This definition of impulsive suicide follows naturally from our study context, and it is also a reasonable definition of impulsive suicide for policymakers and public health officials. Additionally, given that elevated states of suicidal thinking typically only last on average a few hours, the interventions we study that delay firearm purchase by several days will encapsulate most short-term episodes of elevated suicidal thinking (Coppersmith et al., 2023).

outcomes directly to elevated impulsivity compared to nonsuicidal mental patients and healthy controls (Anestis et al., 2014; Conner et al., 2004; Dumais et al., 2005). Additionally, McGirr et al. (2008) finds that the impulsivity-suicidality relationship weakens with age. However, there is an absence of quasi-experimental evidence quantifying this relationship. Motivated by this gap in estimating the relationship between age and suicide impulsivity, we use handgun purchase delay laws as an avenue to investigate how disruptions in impulsive firearm<sup>2</sup> suicide plans may have heterogeneous impacts by age. Leveraging differential timing in the adoption and repeal of purchase delay laws, our difference-in-differences estimates suggest that impulsivity directly increases suicide risk through the sudden ideation of suicide plans that could be disrupted by a “cooling off” period and that suicidal impulsivity in adults wanes with age.

Beginning with Cook and Ludwig (2000), which studied the effect of the waiting period provision of the 1994 Brady Handgun Violence Prevention Act, economists have long identified the beneficial effects of adopting firearm purchase delay laws. The Brady Act instituted a five-day waiting period on handgun purchases from federally licensed firearm dealers between February 1994 and November 1998. While Cook and Ludwig (2000) find an imprecisely estimated negative effect of the Brady Act on firearm suicides, more recent studies leveraging longer panels and more treatment variation (beyond Brady) find evidence of statistically significant declines in firearm suicide as a result of handgun purchase delay laws (Edwards et al., 2017; Luca, Malhotra and Poliquin, 2017). We confirm the direction of these prior findings on firearm suicide across all adults, and demonstrate that the “cooling-off” effect of state-mandated delays in handgun purchase leads to a larger decline in firearm suicides for young adults than for older adults.

Beyond providing the first evidence on the differential impacts of purchase delay laws’ ability to disrupt suicidal plans by age group, our paper brings superior data to bear on this issue while making a key methodological contribution to the literature. First, previous research on the effect of purchase delay laws on suicide, as well as the broader economic literature on firearms and suicide, has primarily relied on state-year panel data (Depew and Swensen, 2022; Lang, 2012). Instead, we obtained restricted access CDC mortality files that enabled us to use county-year as the unit of observation in our panel data, thereby generating more precisely estimated effect sizes than models using comparable state-year panels.<sup>3</sup>

Second, we develop and implement estimators that are robust to heterogeneous treatment effects. In recent years, researchers have developed new approaches to difference-in-differences estimators (Roth et al., 2023) that are not biased by differential treatment effects across groups in staggered treatment adoption settings. We use a stacked regression approach popularized by Cen-giz et al. (2019), which, like the local projection difference-in-differences approach (Dube et al., 2023), is flexible to non-absorbing treatments and specifications with interaction terms as a variable treatment.<sup>4</sup> Additionally, we introduce a novel stacked triple-differences estimator to confirm the robustness of our main findings.

The remainder of this paper proceeds as follows. Section 2 describes the data used to complete

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<sup>2</sup>Our paper specifically looks at firearm suicide, and we do not suggest that our results on suicide impulsivity are generalizable to non-firearm suicides, although they may be.

<sup>3</sup>We show results from comparable state-year panels in the Appendix.

<sup>4</sup>We do not use interaction terms in our main specification but some of our robustness checks do include them.

our empirical analysis. Section 3 presents our methodology. Section 4 presents and discusses our empirical estimates. Section 5 concludes.

## 2 Data

Our county-level data spans all states from 1987-2019, with our sample limited to counties included in the American Community Survey (ACS) in 2019. Our outcome of interest is firearm suicide among adults subdivided into three different age groups, which we measure by aggregating individual-level CDC mortality data to the county-year level. We also create a cause-of-death category for non-firearm suicide. We use the RAND Corporation’s state firearm law database (Cherney et al., 2022) to identify changes in handgun waiting period laws (state-mandated delay in receiving a handgun after the initial intent to purchase), handgun permit-to-purchase laws, and background check laws for firearms purchased from federally licensed dealers. Consistent with the prior literature on handgun purchase delay laws, we consider a state to have a purchase delay regime if it has either an active waiting period law or a permit-to-purchase law.<sup>5</sup> In practice, permit-to-purchase laws always lead to nonzero administrative turnaround time to purchase a handgun. Between the late 1920s and the early 1990s, 21 states introduced handgun waiting period laws, with a minimum of 2 days and a maximum of 15 days, and often accompanied by a background check. In 1994, under the federal Brady Handgun Violence Prevention Act, the remaining 29 states adopted a 5-day waiting period and background check. The Brady waiting period requirement was sunsetted after five years and replaced by the National Instant Criminal Background Check System (NICS) in November 1998. Sixteen states stopped enforcing handgun waiting periods at seven points spanning 1996 through 2015.

Our specifications also include socioeconomic and demographic controls associated with suicide. We collected information on state-year level ethanol consumption from The National Institute on Alcohol Abuse and Alcoholism (Kaplan, 2021a). For our primary regressions, we obtain county-year level covariates (population density, household income, percent in poverty, percent Black, percent 21-34, and percent 35-54) from US Census data via Social Explorer (Census, 2023). Data was linearly interpolated in non-Census years. Our dataset contains approximately the largest quarter of US counties by population and 85 percent of the total US population in 2019.

In supplemental results, we study the impact of household gun ownership on the effect of handgun purchase delay laws, constructing a state-age group estimate of household gun ownership using data from the University of Chicago’s General Social Survey (Smith and Son, 2015) and estimates from the RAND Corporation (Schell et al., 2020) from 1987-2018. For this state-level analysis, we obtain state-age-year-level (household income, percent in poverty, percent Black, percent living in a metropolitan statistical area) covariates from the US Census via IPUMS (Ruggles et al., 2023). More details on our household gun ownership proxy and other data sources can be found in the Data Appendix.

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<sup>5</sup>We round the date of adoption or repeal of these laws to the nearest year.

### 3 Methods

Our approach leverages differential timing in the adoption and repeal of handgun purchase delay laws across US states using a difference-in-differences design. As described by [Goodman-Bacon \(2021\)](#), estimates recovered from staggered-adoption difference-in-difference analyses are a weighted average of individual 2x2 difference-in-difference comparisons. Some of these comparisons, such as using an earlier-treated group as a control for a later-treated group, yield biased estimates if there are heterogeneous treatment effects across treated groups. To overcome this issue in our analysis, our main results use a stacked-regression approach that is robust to heterogeneous treatment effects ([Baker, 2022](#)).

For the estimation of our main results, we first construct a dataset specific to each treatment event,  $h$ , defined as the adoption or repeal of a purchase delay law in a particular year. Each event  $h$ -specific dataset includes all counties whose treatment status was affected by event  $h$  and all clean control countries across a 10-year panel by event time, from  $t = (-5, \dots, 4)$ . Our preferred approach for control groups is to use only never adopters or always adopters, depending on whether the treatment event is the adoption or the repeal of a purchase delay law. We select the control group that matches the treatment status of the county prior to event  $h$ .<sup>6</sup>

We stack all event  $h$ -specific datasets together to calculate an average effect of purchase delay laws across all events ([Cengiz et al., 2019](#)). We employ a Poisson regression model, since a count model is most appropriate for our empirical context. We prefer a Poisson fixed effects model over a negative binomial model because the negative binomial fixed effect approach does not properly account for time-constant variables ([Wooldridge, 1999](#)). Our main specification takes the following form:

$$Y_{it} = \alpha + \beta \text{PurchaseDelay}_{it} + \sum_{j \in M} \gamma'_j X_{it} I(h = j) + \delta_{ih} + \lambda_{th} + \epsilon_{ith}$$

where  $Y_{it}$  represents the number of firearm suicides in county  $i$  in year  $t$ , and  $X_{it}$  represents a set of covariates, and  $M$  represents the set of all stacks  $h$ .<sup>7</sup> The coefficient represents the average estimated treatment effect of adopting a purchase delay law on firearm suicides with stack-specific county and year fixed effects. Standard errors are clustered at the state-stack level, since all purchase delay laws in our sample are changed at the state level, and thus the state is the level at which “random assignment” occurs. In all specifications, we use population as an exposure variable.<sup>8</sup>

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<sup>6</sup>In other words, the control group for adopter treatment-stacks would be the never-treated group. The control group for the repealer stacks would be the always-adopter group.

<sup>7</sup>For our regressions analyzing the impact of purchase delay laws on firearm suicides across all adults aged 21 and over, the covariates are ethanol consumption (measured at the state-year level), the presence of a required background check for firearm purchase from a federally licensed dealer (which applies nationally after 1994, but was in place earlier for 21 states), population density, median household income, percent of people living below the poverty line, percent of people who are Black, the percentages of people within the age groups 21-34, 35-54, and 55 and over, and population as an exposure variable. For our analyses of firearm suicides for a subset of adults, we use all the same covariates, except we do not include the percentages of people within various age groups as controls.

<sup>8</sup>When using count data as an outcome variable, the incidence of the event of interest in an observed group is affected by the size of the group and length of observation; in our context, the larger of two counties with the same suicide rate will see more individual suicides. We choose to include population as an exposure variable simply to constrain its coefficient to less than 1, reflecting our expectation that a one-person increase in population will not lead to a one-incident increase in suicides. In practice, including population as a regular explanatory variable minimally

We estimate the average effect of purchase delay laws on firearm suicides across different age groups using the static model presented above and then provide event-study analyses that allow us to assess the conditional parallel trends assumption. Our event study regresses firearm suicides on a set of yearly dummies for each of the 5 years prior and 4 years after a change in purchase delay laws, omitting the dummy for one year prior to adoption. The following equation shows the regression model underlying the event study analyses, where  $\psi_h$  is equal to the year of the relevant event for stack  $h$  and  $\theta_h$  is equal to 1 if the stack  $h$  pertains to an adoption event and -1 if the stack  $h$  pertains to a repeal event:

$$Y_{it} = \alpha + \sum_{k \in (-5, -4, \dots, 3, 4)/(-1)} \beta_k I[t = \psi_h + k] \theta_h + \gamma' X_{it} \sum_{j \in M} I(h = j) + \delta_{ih} + \lambda_{th} + \epsilon_{ith}$$

The stacked-regression approach relies on a dataset constructed of many treatment event-specific datasets that include only the treated groups and “clean” control groups. Among many new estimators that are robust to heterogeneous treatment effects, the stacked-regression estimator has many attractive properties that make it suitable for our empirical setting. First, the stacked-regression estimator is flexible to the non-absorbing treatment setting and allows us to select the control groups that would be expected to most closely predict the counterfactual path of the treatment group. The estimator is also easily adaptable to include interaction terms to study the treatment’s effectiveness conditional on a second variable. Additionally, the stacked-regression estimator, like many new robust estimators, relies on weaker assumptions in the inclusion of covariates than traditional two-way fixed effects estimators. In particular, the stacked-regression restricts covariate estimation to only the time period of each stack, rather than assuming a uniform estimate of impact of a selected covariate across all group-time observations in the data. Lastly, while the other new estimators that correct for bias in TWFE are restricted to OLS models, the stacked estimator allows for Poisson regression as well.

## 4 Results

### 4.1 Main Specification

We begin by estimating the effect of purchase delay laws on the firearm suicide rate of the entire adult population as well as within three age groups: the young (21-34), middle-aged (35-54), and old (55+) age groups. Table 1 presents these results from both our preferred stacked estimation approach as well as results using a traditional (non-stacked) TWFE estimation approach.<sup>9</sup> The estimates presented in Table 1 and throughout the paper (unless otherwise noted) use incident-rate-ratios (IRRs) for ease of interpretation.

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changes our results.

<sup>9</sup>The non-stacked TWFE estimation follows a specification similar to stacked estimates. The general regression equation for the non-stacked estimation is:  $Y_{it} = \alpha + \beta PurchaseDelay_{it} + \chi'_{it} + \delta_i + \lambda_t + \epsilon_{it}$ .



**Table 1:** Purchase Delay Laws Effect on Firearm Suicide by Age Group, 1987-2019

	(1)	(2)	(3)	(4)
<b>Stacked Results</b>				
Handgun Purchase Delay	0.967* (0.013)	0.939** (0.019)	0.973 (0.019)	0.976 (0.018)
Age	All Adults 21+	Young	Middle Aged	Old
N	25580	25580	25580	25560
	(1)	(2)	(3)	(4)
<b>Non-Stacked TWFE Results</b>				
Handgun Purchase Delay	0.930*** (0.014)	0.905*** (0.020)	0.897*** (0.020)	0.943** (0.017)
Age	All Adults 21+	Young	Middle Aged	Old
N	24849	24849	24849	24849

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Note: County-level Poisson panel data estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II. All regressions use population as an exposure variable.

Because our research design exploits the staggered adoption and repeal of purchase delay laws, a traditional non-stacked TWFE estimator is vulnerable to bias for the reasons stated in Section III. Although the bias can theoretically favor any direction, Table 1 reveals that using a non-stacked estimator substantially overstates the beneficial impact of purchase delay laws on reducing firearm suicides. While our non-stacked results are not directly comparable to those of previous papers studying the impact of purchase delay laws because of our county-level data, Poisson regression specification, and slightly different time period and covariates, our results provide suggestive evidence that prior research on purchase delay laws may have yielded biased estimates due to invalid comparisons embedded within the TWFE estimator.

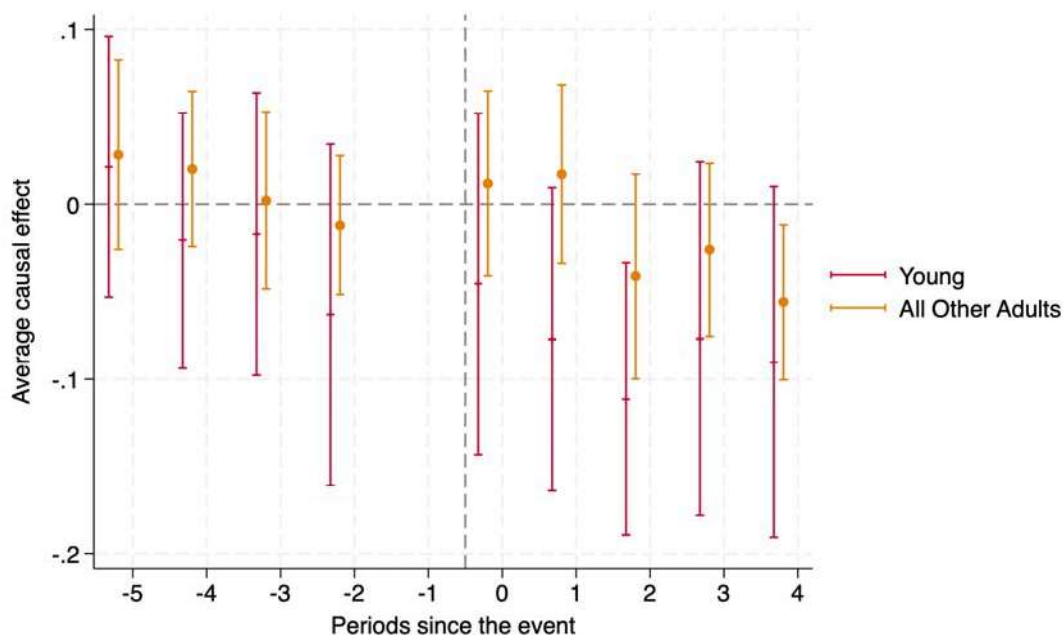
Given the flaws in the non-stacked TWFE estimator, for the remainder of the paper, we only discuss results from our preferred stacked estimator. For the overall adult population, we find that the presence of a purchase delay decreases the incidence of firearm suicide by a modest yet statistically significant 3.3% (IRR=.967). This overall estimate, however, masks heterogeneity of effect size by age, as shown by columns (2) through (4). The overall effect on adults is driven largely by the young age group, whose estimate is almost two times as large as the overall population: adults aged 21-34 experience a 6.1% (IRR=.939) drop in firearm suicide.<sup>10</sup> For middle-aged and

<sup>10</sup>A two sample t-test confirms that the drop in firearm suicides due to handgun purchase delay laws is statistically

older adults, we estimate weaker, non-statistically significant effects. In the Appendix, we show that purchase delay laws have a null effect on non-firearm suicide, providing evidence that there are minimal spillover effects of handgun purchase delays into non-firearm suicides. That is, individuals are not resorting to other means to commit suicide, and the reduction in suicides by purchase delay laws is an absolute decrease in total suicides.

In Figure 1, we also present event plots corresponding to the analyses in Table 1.<sup>11</sup> We find no evidence of non-parallel pre-trends across age groups.<sup>12</sup>

**Figure 1:** Purchase Delay Laws Effect on Firearm Suicide by Age Group, 1987-2019, Poisson Stacked Event Plot



Note: 95 percent confidence intervals with cluster-robust standard errors displayed.

## 4.2 Triple-Difference Estimation

To confirm that our results are not driven by trends in risky behaviors like suicide relative to adoption or repeal of handgun purchase delay laws, we introduce a novel triple-difference stacked regression approach. Our triple-difference analysis compares the effect of handgun purchase delay laws on trends in firearm suicides and arrests made for driving under the influence (DUI). We argue that DUI arrests reflect a risky behavior that may proxy for the underlying level of the self-endangering “acquired capability” for suicide that is described in Joiner (2007). We prefer to use

larger for young adults than it is for middle aged or older adults at the  $p < .01$  level.

<sup>11</sup>To clarify the figure visually, we run event study analyses for young adults and all other adults rather than having four different event study analyses corresponding to the four columns of Table 1. The event studies directly corresponding to each column of Table 1 are qualitatively similar and are available upon request.

<sup>12</sup>The p-value on the F-test of pre-passage dummies is  $p=0.60$  and  $p=0.62$  for the young adults and all other adults respectively.



DUI arrests because they do not vary significantly over time, unlike many other lower-level arrests.<sup>13</sup> Our triple-difference specification assumes that the rate of firearm suicides would have evolved in parallel with the rate of DUI arrests in a county, were it not for the adoption or repeal of a purchase delay. This design flexibly absorbs state-year shocks unrelated to purchasing law changes such as economic conditions or national changes in suicidality.

We aggregate age-specific arrest data from the FBI’s Uniform Crime Reporting (UCR) Program (Kaplan, 2021b) from the agency-level up to the county-level, following a conservative imputation procedure for missing months and dropping agencies that do not report all sample years, as outlined in the Appendix. We estimate the triple-difference using the following equation:

$$Y_{itd} = \alpha + \beta \cdot PurchaseDelay_{it} \cdot I\{d = FS\} + \delta_{ihd} + \lambda_{ith} + \sigma_{dth} + \epsilon_{ithd}$$

where  $d$  represents whether the outcome being observed is firearm suicides (FS) or DUIs.

Results from our stacked-regression approach are displayed in Table 2. The event-study plot is shown in the Appendix. We find that our triple-difference results are consistent with and buttress the findings from our main stacked specification in Table 1. Young adults aged 21-34 see a large, significant drop in firearm suicides of roughly 10% while the depressing effect of purchase delay laws on suicides for older adults is far more muted and not statistically significant.

**Table 2:** Purchase Delay Laws Effect on Firearm Suicide, 1987-2019, Triple Difference Stacked Estimates

	(1)	(2)	(3)	(4)
Handgun Purchase Delay	0.953 (0.034)	0.897*** (0.038)	0.960 (0.038)	0.972 (0.043)
Age	All Adults 21+	Young	Middle Aged	Old
N	31520	31320	31480	31200

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

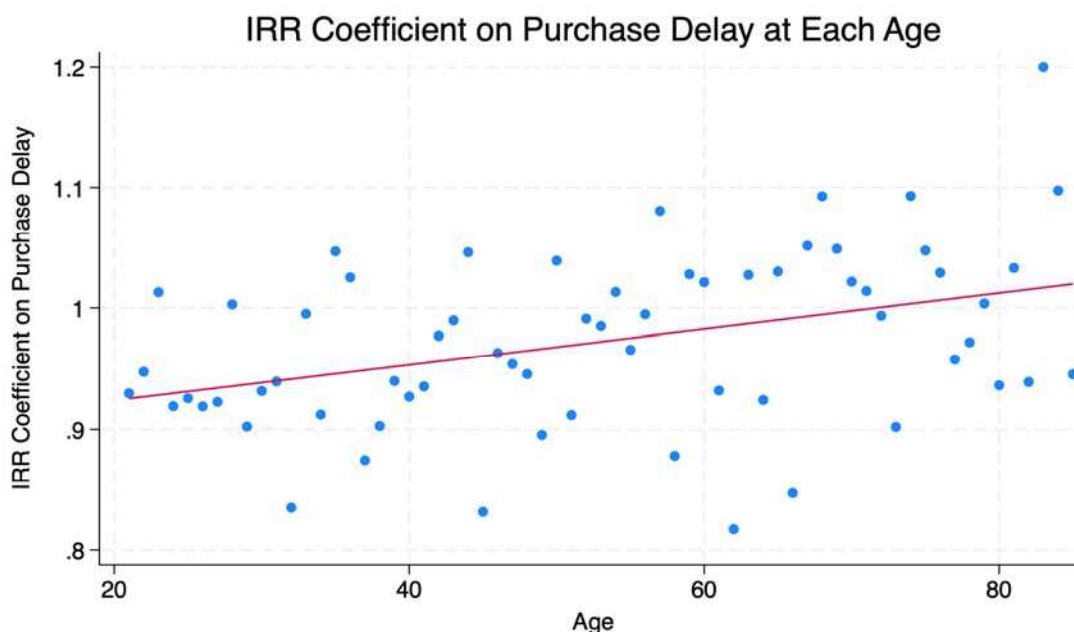
Note: County-level Poisson panel data estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state-level shown in parentheses. Due to lack of DUI observations for treated groups, we do not include the 2001 or 2009 treatment events in our triple difference analysis.

<sup>13</sup>Other potential control groups could have been non-firearm suicides, drug overdoses, or deaths due to alcoholic liver disease. Case and Deaton (2020) have considered suicides, drug overdoses, and deaths due to alcoholic liver disease collectively to be “deaths of despair,” making these other categories of deaths seem like viable control groups. However, using non-firearm suicides in our triple-difference specification would be problematic due to potential spillover effects that would bias results away from zero (although we find no evidence of substantial spillover). Additionally, deaths by alcoholic liver disease are not a practical outcome given our interest in age-specific suicide risk because young people very rarely die of cirrhosis or other alcoholic liver diseases, making this an inappropriate comparison given our interest in age-specific suicide risk. Finally, drug overdoses display significant trends over our study period due to the crack and opioid epidemics. Thus, we opt not to rely on any of the categories traditionally classified as deaths of despair.

### 4.3 Single Age Estimation

The age groups presented in our Table 1 and Table 2 analyses were based on data availability in our *county*-level population data. However, we now show that our results showing the increased effectiveness of purchase delay laws on reducing firearm suicide in young adults are not simply an artifact of these age cutoffs. To do this, we run our preferred specification for each individual age at the *state*-level.<sup>14</sup> In Figure 2, we plot coefficients of purchase delay laws for every age between 21-85 using a state-year panel. The resulting regression line shows that purchase delay laws dampen suicides for the youngest adults by about 9 percent but the effect falls to zero by around age 75.<sup>15</sup> The figure provides compelling evidence of the relationship between impulsive suicide and age. The suicide-dampening effect of purchase delay laws subsides as age increases, demonstrating that however we set age group cutoffs, the effect is most prominent for young people.

**Figure 2:** Purchase Delay Laws Effect on Firearm Suicide by Single Age at State Level, 1987-2019



Note: State-level panel data Poisson estimates with state and year fixed effects, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. All models include covariates as described in Section II, with coefficients reported per single age. All regressions use population as an exposure variable.

<sup>14</sup>We use the same model as we used for our county-level results shown in Table 1. While we still control for socioeconomic and demographic conditions, we use slightly different covariates due to data availability. In the state level regressions, we control for: the presence of a required background check for firearm purchase from a federally licensed dealer, ethanol consumption, percent of individuals living in metropolitan statistical areas, household income per capita, percent of individuals who are Black, and population as an exposure variable. We perform this analysis at the state-level to increase the average population of our geographic unit of analysis since studying single ages will dramatically reduce the population represented by each observation.

<sup>15</sup>The slope of the fitted line is .001 with a p-value of .001.

#### 4.4 Gun Ownership and Effect Size

For purchase delay laws to work, we assume that individuals do not already have a firearm accessible to them and must purchase a new firearm. One possible omitted variable that we are not able to control for in the county-level results is a measure of gun prevalence. It could be that young people are mechanically most impacted by purchase delays because they have a lower level of pre-existing household gun ownership. However, we now show that our results hold at the state-level when we control for gun ownership.<sup>16</sup> We consider how the effectiveness of purchase delay laws vary by *both age and household gun ownership*. We use a dataset at the state-age group-year observation level<sup>17</sup> and run the following interaction models shown in columns 1 and 2 of Table 3, respectively:

$$Y_{stk} = \alpha + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_1 x_2 + \beta_4 x_1 o + \beta_5 x_1 m + \sum_{j \in M} \gamma'_h \chi_{it} I(h = j) + \delta_{skh} + \lambda_{tkh} + \epsilon_{stkh}$$

$$Y_{stk} = \alpha + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_1 x_2 + \beta_4 x_1 p + \sum_{j \in M} \gamma'_h \chi_{st} I(h = j) + \delta_{skh} + \lambda_{tkh} + \epsilon_{stkh}$$

where  $Y_{stk}$  represents the logged firearm suicide rate in state  $s$  in year  $t$  in age-group  $k$ ,  $x_1$  is equal to  $PurchaseDelay_{st}$  as shown in previous equations;  $x_2$  is equal to household gun ownership at time  $t$  for age group  $k$  in state  $s$ ;  $o$  represents a dummy variable equal to 1 for the old age group and 0 for all other groups;  $m$  represents a dummy variable equal to 1 for the middle age group and 0 for all other groups; and  $p$  is a dummy variable equal to 0 for the young adult age group, 1 for the middle-aged group, and 2 for the old age group. The second model assumes an equal gap between the young, middle-aged, and older adults, as is implied by Figure 2, whereas model 1 uses a more flexible approach. Because the unit of analysis is at the state-age bucket level rather than the county-age bucket level, we opt to use a OLS regression instead of a Poisson estimation. We weight our regression by population.

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<sup>16</sup>We note that throughout the 1980s and 1990s, which includes the time period of the Brady Act waiting period that contributes a substantial part of the treatment variation in our data, household gun ownership rates were not meaningfully different for young adults and elderly adults.

<sup>17</sup>Reliable and nationally representative measures of gun ownership are only available at the state level, requiring us to move to the state level rather than the county level. For reference, when weighted by state population, the mean gun ownership in our sample is 37% with a standard deviation of 3.5%.

**Table 3:** Purchase Delay Laws Effect on Firearm Suicide by Age Group and Gun Ownership at State Level, 1987-2019, OLS Stacked Estimates

	(1)	(2)
Handgun Purchase Delay	-0.158*** (0.043)	-0.161*** (0.043)
Gun Ownership	-0.002* (0.001)	-0.002* (0.001)
Middle Effect (Relative to Young)	0.019 (0.037)	
Old Effect (Relative to Young)	0.054 (0.038)	
Gun Ownership x Handgun Purchase Delay	0.002** (0.001)	0.002** (0.001)
Effect As Age Bucket Increases		0.028 (0.019)
Observations	4050	4050

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Note: State-age group level panel data estimates, 1987-2019. Cluster-robust standard errors with clustering at the state level shown in parentheses. Gun ownership is not controlled for separately for each individual stack, but rather a single variable across all stacks, for purposes of presentation. When we control for gun ownership separately by stack, we find similar results on each coefficient. Note that the estimates shown here are not IRRs.

These results suggest that even controlling for gun ownership, purchase delay laws have a strong negative effect on suicides. The results also support the conclusion that the effect weakens as age increases, although using our state-level panel makes it more difficult to precisely measure these effects. Overall, the findings provide evidence consistent with the public health and medical literatures suggesting suicide is an impulsive act for young people and access to a firearm eases barriers for them to commit suicide.

#### 4.5 Further Robustness

In addition to providing evidence supporting the conclusion that our main results are robust to underlying trends in firearm suicides (relative to handgun purchase delay law adoption or repeal), varied age cutoffs, and variation in household gun ownership rates, we perform a battery of further robustness checks to confirm the validity of our findings in the Appendix. We use alternate covariates and clustering variables and find qualitatively similar results. We also show that the effects we

identify are not driven by a few large counties that may be outliers. Further, we show that other demographic variables such as race and gender are relatively stable across young, middle aged, and older adult suicides, indicating that the heterogenous effects we identify by age are likely not due to confounding by another demographic variable. Moreover, our paper, like all prior studies of handgun purchase delay laws, combines the study of both the adoption and repeal of purchase delay laws. In the Appendix, we provide evidence that these effects are roughly reciprocal and can be studied simultaneously. Lastly, we show that our results hold at the state-level.

## 5 Conclusion

Our paper makes three significant contributions to the economics literature on the effect of handgun purchase delay laws on firearm suicide and impulsivity in firearm suicide. First, it adds to the mounting literature documenting bias in estimates from two-way-fixed-effects models and uses a stacked regression approach to address this issue. Our analysis found that this bias was fairly substantial. Specifically, we find that two-way fixed effects models estimate larger effect sizes than those using a stacked regression approach, which suggests that prior literature on the topic may have inflated the magnitude of the benefits of handgun purchase delay laws as a policy intervention in reducing firearm suicide. Second, we use triple-difference stacked estimation to demonstrate the robustness of our main findings, the first paper that we know of to use this technique. Our stacked triple-difference estimator can be flexibly deployed across many empirical settings and overcomes a separate threat to identification that cannot be addressed simply by using a stacked regression difference-in-differences approach. Third, by breaking down the traditional analysis of purchase delay laws and suicide into age groups within our stacked approach, we were able to identify substantial heterogeneity by age group – establishing that the primary benefit of purchase delay laws is to significantly reduce suicides by young adults. In doing so, we are the first study to quasi-experimentally identify and estimate the relationship between age and suicide impulsivity, confirming many hypotheses of the relationship with estimates.

Our paper adds support to the idea, so far understudied by economists, that young people are impulsive in their decision to commit suicide. Our empirical approach also indicates that more research on suicide should test for heterogenous treatment effects by age group. Doing so may provide additional clarity and robustness to researchers as well as important information to policymakers. There are also rich opportunities for further research. For example, to confirm the suicide-impulsivity hypothesis beyond firearm suicide, researchers may look at policies associated with non-firearm suicide by age. Ultimately, further research into understanding how different populations are affected by suicide prevention policies will help policymakers carefully tailor their approach to suicide prevention and more effectively fight this public health crisis.

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